THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. XXXII.—*On the Connecticut River valley Glacier, and other examples of Glacier movement along the valleys of New England;* by JAMES D. DANA.

IN former papers* I have spoken of the existence of a Connecticut valley glacier in the Glacial era; understanding by the expression, as I have said, that the under part of the great continental glacier lying in the Connecticut valley-depression moved *in the direction of the valley*—either while the great glacier was at its maximum thickness and held on its southeasterly course, or after its partial decline.

I propose now to give more fully the evidence with regard to the Connecticut valley movement; and, further, to show that other large valleys of central and western New England had also, in the same sense, their valley-glaciers—that is, they determined the direction of the ice that lay within them.

The facts appealed to in support of the conclusions are partly of my own observation; but they are mostly drawn, for Massachusetts, from the Report on the Geology of the State by Dr. E. Hitchcock, who labored with much enthusiasm in this department of the science; for New Hampshire, from an unpublished map by Prof. C. H. Hitchcock, kindly furnished me by its author; for Vermont, from the Vermont Geological Report, which contains the numerous observations of Prof. C. B. Adams, Prof. C. H. Hitchcock, A. D. Hager and Zadock Thompson; for eastern New York, between New England and the Hudson river, from the volume of the New York Geological

* Manual of Geology, 1863, p. 544; This Journal, II, xxxv, 249, 1863; Trans. Connecticut Acad., ii, 45; This Journ., III, i, 1, and 125.

Reports by Wm. W. Mather, an assiduous laborer in this field of research.

We learn, first, from the scratches on the rocks *outside* of the larger valleys of New England—that is, over its higher lands—that the general course of the continental glacier covering New England was between S. 20° E. and S. 50° E. The true course, deduced from the magnetic, is here given, and so throughout the following pages.

On the high region of western Connecticut (1000 to 1200 feet above the sea), about Warren and Litchfield, the author found the courses of the scratches S. 29° E.; more to the west, east of Kent, on Kent mountain, S. 19° E.; to the south of Kent, about Newtown, S. 38° E. Percival observes that over this western part of Connecticut the direction of the transfer of drift was to the S.S.E. (probably meaning S. 20°–25° E.); and he cites as proof the distribution of blocks of limestone over Litchfield county from Canaan, and also of other rocks over the same and other counties. Mather gives for the direction in Litchfield county, Conn., near Norfolk, S. 20°–25° E., and Hitchcock, for that on Mt. Tom, the highest elevation near Litchfield, S. 17°–22° E.

West of the State of Connecticut, between it and the Hudson river in Dutchess county, not far west of Arthursville, I obtained for the course of scratches (which were common over the region) S. 24° E. Mather found in Putnam county (south of Dutchess), near Patterson, S. 17° E. to S. 22° E.; in Dutchess county, mostly between S. 15° E. and S. 30° E., but in some places S. 35° E.; and north of Dutchess county, in Columbia county (west of Massachusetts), mostly S. 18° E. to S. 30° E., with some on the mountain top east of Shaker Village and elsewhere in that vicinity, S. 45° E.

In the Taconic range, along the western boundary of Massachusetts, Dr. Hitchcock found the course of the scratches on the top of Mt. Everett (2600 feet above the sea) and on the adjoining Mt. Washington S. 18° E.; 15 miles farther north, on top of Tom Ball in Alford, 30 miles north of Mt. Washington, S. 43° E.; on the east slope of the Taconic ridge near Pittsfield (in same latitude nearly with the Shaker village above alluded to) and at Egremont, on the west slope, about S. 50° E.; a little south of the latter, on Lenox mountain, near the road from Richmond to Lenox, S. 38° E. Scratches observed by the writer on Mt. Everett trended S. 27° E.

Again Dr. H. obtained for the course in middle Granville, 20 miles west of the Connecticut, S. 38° E.; between Otis and Becket, 30 miles west of the Connecticut, and farther north in Windsor, about S.E. For eastern Massachusetts (that is, the part of the State east of the Connecticut), Dr. Hitchcock gives as the average direction S. 24° E. He obtained in Royalston, nearly 20 miles west of the Connecticut, S. 18° E. to S. 38° E.

On the higher land of Vermont, away mostly from river valleys, the course of the scratches, according to the Vermont Geological Report, is for the most part between S. 35° E. and S. 55° E. In southern Vermont, on the higher land of Windham, 15 miles west of the Connecticut, S. 40° E.; in Wilmington, S. 29° E and S. 39° E. (intersecting): in central Vermont, in West Hancock, S. 50° E.; in Ripton, S. 60° E. In the northern half of the State, on Camel's Hump, 4088 feet above the sea, S. 55° E.; on Mt. Mansfield, 4430 feet high, S. 55° E.; on Jay's Peak, north of the latter, S. 50° E.; in Stowe, in the valley east of Mt. Mansfield, S. 35° E. Judging from the map in the Vermont Geological Report, which gives some observations not registered in the text, the average course on the higher lands away from the valleys is about S. 50° E.; and the same is not far from the course for the higher lands of New Hampshire, according to Prof. C. H. Hitchcock's map.

The facts show plainly that on the higher lands, both east and west of the Connecticut, and even over the elevated ridges of the Green Mountain range through Vermont, Massachusetts and Connecticut, and also west of this range over the eastern borders of New York, the great continental glacier had a southeastward course; in the latitude of Connecticut, about S. 25° E., and to the northward, S. 35° E. to S. 55° E. With such a course the glacier moved over the elevated lands on the west of the Connecticut river and the elevated lands to the east of that river, keeping onward, with little variation in its main movement notwithstanding the ridges in its course, and following no doubt the general slope of the surface of New England.

But this being true of the movement of its main mass, other facts show that the bottom ice of the great glacier followed often the courses of the valleys beneath it.

1. First as to the *Connecticut river valley ice.* This valley, or great north and south depression of New England, has its termination, as I have elsewhere observed, at New Haven, the Connecticut river channel leaving the valley at Middletown, Conn., and taking thence a southeastward course to the Sound. In the following table, the courses of glacial scratches along the valley are given for comparison with the course of the valley. It commences with localities at the south.

1. Connecticut.	Courses.	Observers.
E. of New Haven bay,	S. 10°–16° W. many	J. D. D.
North of Meriden,	S.	J. D. D.
New Britain,	S. 15° W.	Percival.
Mt. Carmel, 7 m. N. of N. H.	S. 30° W.	W. B. Dwight.
Wadsworth's mountain,	Southwesterly	Mather.
2. Massachusetts.		
Granby, 7 m. E. of Conn. R.	South nearly	E. Hitchcock.
Mt. Tom,	South "	"
Mt. Holyoke,	South, S. a few degrees W.	"

Massachusetts.	Courses.	Observers.
Sunderland, E. side of Conn. R.	South nearly	E. Hitchcock.
Deerfield, S.E. part,	South "	"
Montague, E. side of Conn. R.	South "	"
Greenfield, W. " "	South "	"
Northfield, E. " "	South "	"
3. Vermont.		
Vernon, for 2 m. W. side of Conn. R.	S.	C. H. Hitchc'k
Guilford, 5 m. W. of Conn. R.	S. 8° E.–S. 13° E.	C. B. Adams.
Brattleboro, for a great distance along C.R.	S. and S. 8° E.	C. B. A.
Dummerston, near Conn. R.	S.	C. H. H.
Putney,	S. 5°–12° W.	C. B. A.
Rockingham,	S. to S. 2° W.	C. H. H.
Norwich, 2 m. W. of village,	S.	C. H. H.
Norwich,	S. 15° E. and S. 39° E.	
Thetford, W. part of town,	S. 9° E.	C. H. H.
East Fairlee,	S. 6° E.	C. H. H.
Bradford,	S. 19°–30° E.	C. H. H.
"	S. 30° E.	C. B. A.
Newbury,	S. 12°–30° E.	C. B. A.
Waterford and Barnet,	S. 5° E. many; also S. 8° E.	

In New Hampshire, the courses of scratches, as represented on the map of the State by C. H. Hitchcock, referred to above, correspond very nearly with those of Vermont. According to it, scratches trending *west* of south occur in the river towns Chesterfield, Walpole, Hanover, Oxford, Haverhill, and still farther north, along the Passumpsic, in Monroe and towns adjoining it on the east.

Now the average course of the whole Connecticut river valley and of the Passumpsic valley, its true continuation northward, is about S. 9° W. But its different parts vary from this; that north of Massachusetts trending about S. 12° W.; that in Massachusetts, S. 6° W., but varying in the southern part to S. nearly; that in Connecticut from the Massachusetts line to Hartford, S. 3° W.; that from Hartford to New Haven, S. 18° W.; and the whole average for Connecticut, S. 15° W.

The table shows that the courses along the valley from New Haven to White River Junction or Norwich, Vt., are between S. 16° W. and S. 9° E., instead of between S. 30° E. and S. 50° E., like the scratches over the higher lands; and that, therefore, the average difference between the general course of the glacier and its course along the Connecticut valley is over 30°, that is, in the valley there are upward of thirty degrees more of westing.

In Connecticut, in which the average course of the valley is S. 15° W., the common course of the scratches is S. 15°–16° W.

In Massachusetts, in which the valley trends S. to S. 6° W., the scratches trend a few degrees west of south on Mts. Holyoke and Tom, or in part at least, as stated by Hitchcock; and as these are much the highest points in this part of the valley (their tops 1126 and 1211 feet above the sea level), they afford

consequently the best evidence of the average direction of the movement in that region.

In Vermont, where the course of the valley for the more southern half is S. 12° W., the scratches trend S. to S. 13° E. But north of White River Junction the course of the scratches varies between S. and S. 30° E., yet many scratches at the farthest point, near the mouth of the Passumpsic, are S. 5° E. to S. 8° E.

In the part of the Connecticut valley south of Vermont the scratches conform closely in direction to the trend of the valley, and are the only scratches; while to the north there is a general southerly course in the scratches of the Connecticut river valley, yet at the same time about 15° less of westing than in the average trend of this part of the river. Moreover, in this upper part of the valley there are often, besides the valley set of scratches, another set having the southeasterly course of the great glacier.

The width of the region bearing the north-and-south scratches of the valley is generally twenty to thirty miles, but sometimes more. Going east or west of this there is a change more or less gradual to the course of the great glacier, and often also other scratches conforming to its course occur. In Massachusetts, in Heath, 15 miles west of the Connecticut, the course of the scratches given by Hitchcock is south with some westing, and the same on Mt. Pocomptuck in this town, 1888 feet high; and in Rowe, 20 miles west of the river, the course is S. 2° W. In southern Vermont, in Halifax, west of Vernon, and 10 miles west of the Connecticut, the directions given by Hitchcock are mostly S. 12° W.; but also, in *West* Halifax, 15 miles from the Connecticut river, S. 53° E.; in Marlboro, north of Halifax, S. 20° E. on high land; and also, at another locality, two courses, S. 7° W. and S. 53° E., intersecting.

The facts show beyond question that *the abrading agent of the Connecticut valley moved mainly in the direction of the valley.* The less degree of conformity of direction between the scratches and the valley in its *upper* portion is what should be expected; and so also the intersecting southeasterly scratches of this portion, and also of the region on either side of it a little distant from the river.

2. *Upper Champlain Valley.* The main part of Lake Champlain is the region of a vast plain—though a plain of water—and little or no conformity to its direction in the course of the scratches on its borders could be expected. At Burlington the courses are S. 19° E. to S. 31° E.; in Shelburne, south of Burlington, S. 17° E. to S. 29° E.; which courses show apparently that some diverting effect was produced by the main ridge of the Green Mountains. But the southern prolongation of the lake from Addison, and even from Ferris, southward to White-

hall, a distance of 40 to 50 miles, is very narrow, and occupies a proper valley, and here the scratches are parallel mostly to the trend of the lake—which trend is nearly north and south, excepting for the southern part, where it is about 16° *west* of south (S. 16° W.). Along this more southern portion, in Benson, the course of the scratches, according to the Vermont Report, is S. 8° E., S. 12° W., S. 15° W.; and in Orwell, just north, S. 8° E., S. 12° W. Farther north, in Bridport, the course is S. 20° W.; in Addison, S. 17° W. and S. 13° E.; at Larrabee's Point, S. 4°–12° E.; at Crown Point in New York, opposite to Bridport, S. 2° E. on the west side, and S. 27° E. on the east. In Putnam, N. Y., west of Benson, according to Mather, S. 10°–15° W. The conformity of the course of the scratches to the trend of the valley in its southern half is very close; in its more northern part, where, opposite Addison and beyond, the lake is over two miles wide, scratches both of the valley course and of the general course of the glacier occur together. The divergence in all, from the S. 40°–55° E. of the higher lands of the State, is very wide.

3. *Lamoille River Valley and Winooski River Valley.* These rivers, in the northern half of Vermont, rise in the *eastern* part of the State and pass through the Green Mountains, the former north of Mt. Mansfield, the latter just north of Camel's Hump; in the case of each, the bottom of the valley where the river passes the line of these mountains is not far from 4000 feet below their summits. The course of each is about N. 15°–20° W. and S. 15°–20° E.

Along each of these valleys, the glacial scratches are closely parallel to its main trend, as shown and recognized by the Vermont geologists. It is strikingly exhibited on a map of the State accompanying the report. On the Lamoille there are the courses S. 55° E. to S. 85° E.; on the Winooski, the courses S. 60° E., S. 80° E., and even east-and-west in one case.

4. *Otter Creek Valley.* Otter Creek flows northward along the great valley in the southern half of Vermont, just west of the axis of the Green Mountains, passing by Rutland, Brandon, Salisbury, toward the southern end of the broader part of Lake Champlain. Its general course is about N. 15°–20° W. and S. 15°–20° E. The glacial scratches in the valley have the courses S., S. 12° E., S. 15° E., S. 20° E.

5. *Merrimack River Valley* (New Hampshire). The conformity between the courses of the scratches along this valley above Lowell and its trend is well shown on the map of New Hampshire by Prof. C. H. Hitchcock. The average trend of the valley is but a little east of south, and the same is true of the scratches.

We have thus evidence of the existence during some part of the era of ice not only of a glacier movement in New England

along the Connecticut river valley, but also of one along the upper Champlain valley, the Lamoille valley, the Winooski valley, the Otter creek valley, and probably the Merrimack valley; and many of the courses of scratches observed in other parts of Vermont and New Hampshire have divergences from the normal course of the great glacier, which are probably due to the valley-depressions of the surface. Among these smaller valleys are perhaps those of the Queechee, Black, Middlebury and White rivers of Vermont and the Deerfield of Massachusetts; for the existence of an *independent* glacier in each of these valleys is recognized as probable by Prof. Hitchcock, on the ground of the conformity between the direction of the scratches and the valley, although the iceberg theory is adopted by him for all the rest, even the Lamoille, Winooski and Connecticut. The writer has elsewhere mentioned the evidence in favor of a Hudson river glacier movement, and of another in the Mohawk valley running easterly through central New York; and further, of one along the St. Lawrence valley, the scratches in it following its course according to the observations of Dr. Dawson.

The facts are sufficient to prove that examples of valley movements of glacier ice must have been common over the continent in the Glacial era, or rather the rule for all the larger valleys. It is hence evident that no observations on glaciers are complete which do not take into account the surface features of the country. Prof. Hitchcock recognized the deflecting influence of the Connecticut and Lamoille valleys; but the deflection, in his view, was that of icebergs. Besides the argument against the iceberg hypothesis elsewhere presented, New England affords another in the fact that if there were, at the time, a submergence to the depth required to overcome the obstacles to a southeast movement offered by the southerly trend of the mountains, there would still have been no southeasterly flow of waters over its surface, since no currents having this course exist in the adjoining seas. And further, the necessity of helping out the iceberg theory by bringing in glaciers for some of the smaller valleys, because icebergs could not be supposed to have worked their way along them to do the scratching, affords another strong argument against it.

The question now comes up, whether the scratches in the valleys were made in the Glacial era while the glacier was of quite or nearly its maximum thickness, or during the decline of the glacier, when its thickness was so diminished as to make the ice of the valleys essentially independent glaciers.

With regard to the Connecticut valley ice, the following evidence appears to show that the movement took place while the continental glacier still had its continental, or at least its New England, movement. *a.* In Massachusetts and Connecticut the

only scratches in the valley are those trending south or *west* of south, and they are often very deep; even the tops of Mt. Tom and Mt. Holyoke have only these scratches. This uniformity seems to prove that the direction of movement thereby indicated characterized the ice of this part of the valley through *the whole of the Glacial period.*

b. Again, if a local glacier occupied the valley having a thickness of say one, two, or three thousand feet, or such as would lie below the level of the Green Mountain summits, the glacier would have had through its breadth a nearly southerly course corresponding to the trend of the valley, and in that case southerly scratches should have existed over the whole surface, even localities remote from the Connecticut river—*where they are not found.*

In another place I have supposed that the southeasterly course which occurs in the scratches to the west of the Connecticut river might have been a resultant between the tendency to a southerly movement down the valley, and that down the slope into the valley. But this was so only to a very small degree. For the ice, after passing over the valley, resumed on the east its southeastward scratching.

c. In the part of the Connecticut valley north of Massachusetts, the course of the scratches is not that of the valley, but differs 10° to 15° from it to the *eastward.* This greater easting shows that the southerly movement of the ice induced by the valley was modified by some force pressing it eastward, and this force could only have been that due to the movement of the general glacier. Hence it proves that the general glacier existed at the time in nearly or quite its full force.

We may conclude, therefore, that the valley ice of the Connecticut had through its southern half (across Massachusetts and Connecticut) its own independent southward motion, mostly unmodified, during the whole progress of the Glacial era. But along the more northern part of the valley, there were the modifications in the valley movement just pointed out, and also scratches made by the general glacier. If the southeasterly scratches (those of the general glacier) should be found to be the older of the two sets, we should infer that when they were made the general glacier was then the thickest; but the facts would not prove that the thickness had been so far diminished as to leave the Connecticut valley glacier wholly an independent one. Prof. Hitchcock has stated as his belief that the southwesterly scratches were the oldest, but admits that there is much doubt with regard to it.

This movement of the bottom of a glacier six or eight thousand feet thick along a different course from its main mass wherever it lies in great valleys, is a necessary result of mechanical law. It moves just as thick pitch poured over a slop-

ing surface in which there are a few large groovings would move, the mass following the general surface, and the portions in the grooves nearly or quite the course of the grooves. The thickness of the ice that followed the course of the valley was at least 2000 feet; for the southerly scratches occur not only on the summits of Mt. Tom and Mt. Holyoke, but also on the top of Mt. Pocomptuck in Heath, 15 miles west of the Connecticut, the height of which, as stated, is 1888 feet.

As to the Lamoille, Winooski and Otter creek valleys, the case is somewhat different; for if the movement of the ice in the valleys took place when the great glacier was in full force, it was a movement in each *up the valley*. The valleys are, however, of gradual slope—that of Otter creek extremely gradual—and facts show that the ice is often moved up slopes many hundreds of feet. Dr. A. S. Packard, Jr., mentions the occurrence of fossils in the drift on Mt. Katahdin (Maine) at a height of 4000 feet above the sea, which must have been brought from the low country to the north where such fossils are found in place. As Dr. Packard observes, such facts show that icebergs were not the transporting agents.

It is, however, possible that each of these three valleys had its independent glacier during the melting of the ice. But if so, the great glacier must have been removed wholly out of the way of the valley glacier, so as to be no impediment to its movement down the valley; and it seems probable, from the small extent of these valleys, that by the time the general glacier had got out of the way, the valley ice would also have mostly disappeared. It is quite probable that a careful study of the scratches along one or another of these valleys may decide this interesting question.

It seems to be a natural consequence of a gradual melting of the great glacier, that sooner or later parts of the ice should have become independent and taken independent movements. We should naturally look for this independence at least in the large valley of the Connecticut river. A thinning of the ice to 2000 feet would necessitate, it would seem, a valley movement southward. And yet, as has been observed, the evidences of such a glacier movement do not exist—that is, the scratches are confined to so narrow a band along the center of the Connecticut valley as to show that they were not made by an independent Connecticut valley glacier. How then can we reconcile the fact that the ice must have thinned down to 2000 feet, 1000 feet, and so on, and yet had no movement as a separate glacier? The explanation is this:—The melting of the ice took place in the early part of the Champlain era—an era of subsidence for New England and a large part of the continent (and therefore favorable to the melting); and this subsidence was greatest in New England to the north, having been at least

325 feet in the latitude of Burlington; nearly 150 feet near the northern boundary of Massachusetts, one hundred miles from the termination of the valley (the highest terrace on the river in Hinsdale, New Hampshire, being 159 feet above the river, according to the Vermont Report); and 45 to 50 feet at New Haven, the southern end of the Connecticut valley. Such a subsidence would have diminished the average slope of the whole valley about *one-and-a-half feet a mile.* For the southern half, from northern Massachusetts to Long Island Sound—100 miles—the slope, which now averages *two* feet a mile, would have been reduced to *one-and-one-tenth* of a foot a mile; and from Springfield down, which is 60 miles from the Sound, and the height of the water level only 64 feet above the Sound, making the average slope below about one foot a mile, and where the Champlain subsidence was at least 60 feet more than at New Haven on the Sound, there would have been no appreciable slope in the waters; the basin, as it is designated by Hitchcock, from Middletown in Connecticut to Holyoke in Massachusetts, would have been strictly a basin.* Under such circumstances the ice along the valley would have lost all motion. The same condition of rest would have belonged to the ice of other north-and-south valleys of as small rate of descent; but not necessarily to east-and-west valleys like the Lamoille and Winooski.

High up even the north-and-south valleys, where the slopes were not sensibly changed by such a subsidence, local glaciers might well have existed. Evidences of them in the region of the White Mountains have been pointed out by Dr. A. S. Packard, in an excellent memoir on the Glacial Phenomena of Labrador and Maine (94 pp. 4to), published in Volume I of the Memoirs of the Boston Society of Natural History (1867); also by Professor Agassiz, in the American Naturalist for November, 1870, who states that he observed the marks of local glaciers in the White Mountain region in the year 1847, soon after his arrival in this country.

In the foregoing pages the facts from the State of Maine have not been referred to. These are well discussed by Dr. Packard in the memoir just referred to, in which he recognizes and applies the principle discussed in this and the writer's former papers on the valley glaciers. He observes that of the eighty

* In order to deduce the amount of subsidence for any place on the river from the height of the highest terrace above the ordinary level of the river, it is necessary to deduct first the height of the lower flats. This would give for the amount at Hinsdale 138 feet, which is 88 to 93 greater than at New Haven; and for the amount at Springfield, where the highest terrace is 136 feet, it would give 115 feet, or 65 to 70 feet greater than at New Haven. Such calculations may be in error, and generally will give less than the actual amount, because the height of the terrace depends on the amount of *excavation* that has taken place since the land reached its present level; and this is in most cases less than the amount of elevation.

localities of scratches that have been noted in Maine, the scratches in sixty-two have a southeasterly course: that the southeasterly course of the glacial grooves and striæ is especially marked in the interior of the State on the high lands and low mountains; but, approaching the coast, the evidence shows that the glaciers moved down the river valleys, and thus assumed a more north-and-south course, and at times, owing to local trends in the depressions, were even deflected so as to flow in a direction a few degrees west of south. The facts in Maine are just such as are general to New England.

The same principle is recognized by Prof. N. S. Shaler in the Proceedings of the Boston Society of Natural History, for 1870. Other similar facts have been recently pointed out in States to the west of New England. When the applications of the principle are studied out over the whole continent, we shall understand better than we now do the sources of the varied movements in the great glacier.

Art. XXXIII.—*The Paragenesis and Derivation of Copper and its associates on Lake Superior;* by Raphael Pumpelly.

II. *Paragenesis of the Minerals associated with Copper.*

No. 1. Capen Vein.—This is apparently a true fissure vein. It occurs in a compact and very tough melaphyr, which is exceedingly chloritic near the vein. All the joints within a distance of several yards from the vein are covered with a coating $\frac{1}{10}$ to $\frac{1}{2}$ inch thick, of dark-green and bluish-green chlorite, having a combined fibrous and foliated structure oblique to the joint surfaces. The melaphyr is rich in magnetite. Sheet copper was found in mining, but not in paying quantity.

1. *Laumontite*, in thin seams.
2. *Prehnite*, in seams which cut through those of laumontite, also between symmetrically arranged bands of laumontite.
3. *Chlorite*, as destroyer and replacer of prehnite, and as lining of cavities in the latter.
4. *Analcite*, in clear crystals on the prehnite and chlorite.
5. *Calcite.*

No. 2. Huron Mine.—1. *Laumontite*, in thin crystalline bands on the sides of a cavity; the free ends of the opposed crystals nearly meet.

2. *Prehnite*, filling the space between the bands of laumontite.

No. 3.* Copper Falls Mine.—Fissure vein. 1. (?) *Natrolite.* 2. *Laumontite.* 3. *Analcite.*

No. 4.* Same vein.—1. *Apophyllite.* 2. *Copper.* 3. *Orthoclase.*

* Taken from a list given by Hilary Bauerman, Quart. Journ. Geol. Society, Nov., 1866.

No. 5.* Bay State Mine.—1. *Prehnite.* 2. *Quartz.* 3. *Copper.* 4. (?) *Laumontite.*

No. 6.* Phœnix Mine.—Fissure vein. 1. *Laumontite.* 2. *Quartz.* 3. "*Green-Earth.*"

No. 7.* Bay State Mine.—Fissure vein. 1. *Quartz.* 2. *Apophyllite.* 3. *Calcite.*

No. 8.* Bohemian Mine.—1. *Analcite.* 2. *Copper.* 3. *Orthoclase.*

No. 9. Amygdaloid Mine.—Fissure vein. 1. *Prehnite*, in its characteristic reniform shape.

2. *Quartz*, in small crystals on the prehnite.

3. *Analcite* crystals, covering the quartz.

4. *Orthoclase* crystals, on the analcite and quartz.

No. 10. Bay State Mine.—Fissure vein. On the soft brown gangue. 1. *Analcite*, lining part of a vugg. The crystals are $\frac{1}{4}$ inch in diameter, often white and transparent, but very much fractured. Near the contact with the rock they are often reddened internally and much altered, and then surmounted by the next member.

2. *Orthoclase*, in the usual minute crystals, some of which are scattered over the altered analcites.

No. 11. Amygdaloid Mine.—Fissure vein. 1. *Prehnite*, in the characteristic reniform shape, forming the body of the specimen; fresh-looking on the free surface, but on the under broken side somewhat porous, with earthy fracture, and then rather intimately associated with datolite and a soft green (chloritic?) mineral.

2. *Copper*, in films traversing the prehnite, and molded to the reniform surface. While the under surface of the copper bears the impression of the prehnite, the upper surface, now free, bears that of some mineral that is gone; threads of copper rising from this free surface $\frac{1}{4}$ inch are crystallized at the tips, where they stood above the mineral that has disappeared.

3. Minute grains of a hard yellowish-white mineral, sprinkled like meal over the prehnite and copper; under the microscope appear to consist of sheaf-like clusters of minute rhomboidal plates; fuses with difficulty.

4. *Datolite*, in microscopic crystals on No. 3; others, one line in diameter, rosy, with suspended flakes of copper, lie upon the prehnite.

No. 12. Amygdaloid Mine.—(Fissure vein.) On the gangue—here chloritic—lie, 1. *Calcite*, imbedded between the gangue and No. 2.

2. *Prehnite*, forming the greater part of the specimen, and having a tolerably fresh luster.

3. *Copper*, in grains, flakes and threads conforming to the radiating cleavage planes of the prehnite.

4. *Datolite;* compact amorphous, white translucent mass, covering the prehnite with a layer of which $\frac{3}{4}$ inch thickness, still remains. The copper threads do not penetrate it.

No. 13. PEWABIC COPPER-BEARING BED.—This specimen—about $2\frac{1}{2}$ inches by $3\frac{1}{2}$ by $\frac{3}{4}$—is evidently from the interior of a druse, to whose wall it was attached by only a small part of its surface. The body of the specimen is copper, very cavernous, much of it pseudomorphous after laumontite. The copper is very thickly bestrewn with small green crystals of quartz—prisms terminated at both ends,—which are however *older* than the copper. On the sides and around the edges of the specimen there are beautifully modified scalenohedrons of calcite. The successions are:

1. *The rock or mineral* to which the laumontite was originally attached, and which has disappeared.

2. *Laumontite* or leonhardite; has also disappeared; the prisms were $\frac{1}{8}$ to $\frac{1}{2}$ inch long, terminated at one end with a hemidome.

3. *A mineral,* now gone, which must have been present to support the quartz crystals (see Quartz). It may perhaps have been the alteration-product of the laumontite or an enclosing mineral; laumontite crystals occur frequently enveloped, except the base, in calcite.

4. *Quartz,* in prisms $\frac{1}{32}$ to $\frac{1}{8}$ inch long, often terminated at both ends. They occur on parts of nearly every one of the pseudomorphs after laumontite; the copper is moulded to them, giving casts even of the striæ on the prisms, and they frequently pass entirely through the pseudomorphs after laumontite, so that the two ends of a quartz prism frequently just appear on opposite sides of the pseudomorph and transmit light. In some instances the quartz crystals are so numerous as to touch each other, but they are often wholly isolated, and supported only by the copper which is younger.

The quartz crystals contain minute, brilliant particles of copper, wholly isolated, in the interior.

5. *Copper,* in the form of laumontite, preserving often the sharpness of the angles and smoothness of the faces of the original mineral, when seen by the naked eye; under the glass the surface is less even. The pseudomorphs are not solid copper, as will appear in describing other specimens.

6. *Chlorite?* a soft, light-green mineral in minute hemispherical forms, with radiating structure, scattered over the quartz and copper. Wherever this mineral lies upon the quartz crystals, these are more or less penetrated by it, and some of them are eaten through and through to such an extent that the crystalline form is no longer recognizable.

7. 2*d Copper Calcite;* Calcite crystals—scalenohedrons—$\frac{1}{8}$ to $\frac{3}{4}$ inch long, lie on the sides and around the edges of the specimen. These crystals, in forming, adapted themselves with partially entering faces to the rough surface of the preëxisting quartz and copper. Some of the calcite crystals contain brilliant isolated films of copper, which must have been formed at least after the calcite had begun to crystallize, and is therefore younger than the copper previously mentioned.

8. *Datolite*, in exceedingly minute crystals, lying on both the chlorite and calcite; they are less than $\frac{1}{200}$ inch in diameter, but the datolite form is distinctly visible under the microscope; they fuse easily with the characteristic green flame.

No. 14. PEWABIC COPPER-BEARING BED.—1. *The rock* or mineral to which the laumontite was originally attached, which has disappeared, copper now forming the support of all the members.

2. *Laumontite*, of which only the form now remains.

3. *A mineral*, now gone, which seems necessarily to have been present to support the isolated crystals of quartz.

4. *Quartz*, in minute prisms, containing brilliant particles of copper.

5. ? *Calcite*, represented only by impressions in the copper. This calcite may, perhaps, be older than some of the foregoing members.

6. *Copper*, now forming the body of the specimen. It is very cavernous, and besides forming in places pseudomorphs after laumontite, it is the support of every member of the series. That it is younger than the quartz crystals is shown by the fact that on removing these, perfect casts of them are visible in the copper. The copper also contains impressions of calcite crystals (see above).

7. *Chlorite?* the same mineral as the 6th member of No. 13, and occurring in the same manner.

8. *Calcite;* a few small scalenohedrons planted on the copper in the impressions of the older calcite = 5th above.

No. 14*a*. *Copper after laumontite*, from the PEWABIC COPPER-BEARING BED.

The upper face of this specimen is part of a partially filled cavity in a cupriferous and highly altered amygdaloid; the lower, or broken face, is a portion of the altered amygdaloid itself. The general appearance of the specimen at first glance is that of a drusy cavity, nearly filled, except in the middle, with broken crystals of calcite, whose interiors contain many thin plates and threads of native copper.

The amygdaloid is a soft compact brown and green rock with earthy fracture,—an altered amygdaloidal melaphyr. The small amygdules near the wall of the larger cavity are of calcite.

The pseudomorphs of copper after laumontite are prisms $\frac{1}{8}$ to $\frac{1}{2}$ inch long and about $\frac{1}{8}$ of an inch square, and are terminated with a hemidome; they are each attached by one end to the wall of the cavity and project out toward the interior. The angles are often sharp, though in some instances the junction of two faces of a prism presents something of the appearance of a copper cast made in a mould whose two halves fit only imperfectly together. Sometimes, under a strong glass, the joining is clearly imperfect, and the pseudomorph has the effect of a prism built with four badly-soldered plates of metal.

Minute prisms of quartz (colored green by the chlorite-like mineral mentioned in specimens No. 13 and 14) project from the interior of the pseudomorphs, through the copper, to $\frac{1}{100}$ of an inch above the surface.

In one place I cut to the depth of $\frac{1}{12}$ of an inch in solid copper; but a cross fracture in another prism showed that the copper was, there, a mere superficial film, while the interior was occupied by a confused and rather porous mass of quartz prisms, copper, and the green chlorite-like mineral mentioned above. It is these quartz crystals whose ends just pierce the copper coating. In some instances a prism of quartz terminated at both ends passes entirely through the pseudomorph and appears on both sides, and allows the light to pass through. After removing a quartz crystal from the copper, a perfect cast, even to the horizontal prism-striæ is found in the copper.

The copper-surface of the pseudomorphs seems nearly smooth to the naked eye, but under a strong glass it appears not only often perforated with holes, but it often shows flakes of copper rising on edge to a height of $\frac{1}{100}$ of an inch above the face.

These pseudomorphs before the breaking of the specimen were imbedded in the interior of scalenohedrons of calcite, except at the attached ends of the prisms. At the contact planes between the calcite and the pseudomorphs, the former seems to adapt itself fully to all the irregularities of surface of the latter.

On the bottom of the specimen the calcite amygdules exhibit marked signs of change to datolite. The transparent crystals become gradually opaque, with a pearly lustre on the cleavage planes, and a little farther away this condition merges almost insensibly into a lustreless white mass composed of an aggregation of exceedingly minute crystals, which exhibit the datolite form under the microscope, and fuse easily with the characteristic green flame before the blowpipe. The same change is visible, in places, on the crystals of calcite enveloping the pseudomorphs after laumontite.

The relative ages here appear to be, 1, *The amygdaloid*, though probably not in its present condition; 2, *Laumontite*; 3, *Quartz*; 4, *Copper, chlorite-like mineral*; 5, *Calcite*; 6, *Datolite.*

No. 14 (*b*) *Another specimen* from the same locality exhibits,—besides pseudomorphs of copper after laumontite,—pseudomorphs of quartz after laumontite. In these last, the ends of the pseudomorphs are broken off, leaving only the prism. The faces are formed by a tolerably even mass of quartz, on the outer surface of which a crystalline form appears here and there under the glass. The interior of the prism is not a compact mass of quartz, but is nearly filled with quartz prisms projecting from all sides toward the middle and containing minute brilliant and isolated particles of copper.

Near these are the copper pseudomorphs; they are mere hollow shells, scarcely as thick as paper; the angles are sharp and the faces tolerably smooth, but often pierced with holes. The hemidome of one of these is studded with the ends of minute quartz prisms, which occupy the interior of that part of the pseudomorph and project through the copper shell.

In this specimen also, some of the pseudomorphs are imbedded in scalenohedrons of calcite, which sparkle with brilliant particles of copper swimming in the transparent crystals.

In these remarkable pseudomorphs the quartz is undoubtedly the oldest existing intruder, while the copper, which far more generally preserves the crystalline form of the laumontite, seems to be pseudomorphous in, at least, the second degree of removal. Yet in nearly all instances the older quartz is present, occupying a part of the space originally belonging to the laumontite crystal; and very often these quartz crystals are wholly separate from each other and supported only by the younger copper. Something which is now gone must have existed to perform this office of support before the copper was deposited.

No. 15. "Ragged Amygdaloid," Albany and Boston M.—On the rock lie: 1, *Prehnite*—2, *Orthoclase*, in minute crystals on the prehnite.

No. 16. "Epidote Lode," St. Mary's.—In a cavity in the quartz-epidote rock, which forms a frequent feature of this bed, lie: 1, *Prehnite* crystals, disposed as a reniform lining of the cavity—2, *Quartz*, in transparent prisms on the prehnite—3, *Analcite*, crystal $\frac{1}{2}$ inch in diameter, slightly opaque and somewhat cavernous internally, planted on the quartz.

4. *Orthoclase* crystals planted on the prehnite, quartz and analcite.

The prehnite is partially altered, containing cavities lined or filled with a soft green mineral, chlorite or green-earth. There is also a greenish-yellow chlorite-like mineral, which incrusts and has eaten away the surface of the quartz crystals.

No. 17. Amygdaloid on the Kearsarge Location.—On the rock lie: 1, *Prehnite*—2, *Quartz* on the prehnite.

No. 18. HURON MINE.—On the rock lie: 1, *Analcite*, in a continuous band $\frac{1}{2}$ inch thick, crystallized on the inner face; it is reddish and perhaps much altered, though still hard—2, *Calcite* incrusting the analcite crystals, and occupying cavities in their interior.

No. 19. ALBANY AND BOSTON AMYGDALOID.—The rock of this bed is a wholly irregular mixture of hard light-green amygdaloid and soft brown amygdaloid, in which the vesicular form is frequently lost from the fact that the cavities containing secondary minerals have extended and become merged together, forming a confused patch and vein structure: 1, *Prehnite*, amorphous and altered to a slightly cavernous appearance on the surface—2, *Quartz* in prisms—3, *Orthoclase* in minute crystals chiefly on the altered prehnite, with which its formation is probably connected, and also on the quartz.

No. 20. SAME BED.—On the amygdaloid, which contains quartz amygdules, lie: 1, *Prehnite* penetrated with strings and films of copper—2, *Quartz* in prisms; *chlorite-like mineral* in hemispherical forms, with radiating structure—*Orthoclase* in minute crystals; all these lie separately on the prehnite—3d, *Calcite* covering all the above mentioned members.

No. 21. SAME BED.—1, *Quartz*, in prisms—2, *Chlorite-like mineral* in hemispherical forms, with radiating structure; wherever it is in contact with the quartz it has pitted it and eaten into it—3d, *Calcite*.

No. 22. SAME BED.—On the amygdaloid lie:—1, *Prehnite* crystals in reniform masses—2, *Quartz*, in prisms on the prehnite crystals—3, *Orthoclase; Calcite;* the orthoclase is in minute crystals on the prehnite and quartz.

No. 23. SAME BED.—The amygdaloid on which the following succession occurs consists of quartz and chlorite, and is wholly altered—so much so that the quartz which now composes a large part of it is evidently of the same age as that which follows the prehnite—1, *Prehnite*, in crystalline reniform masses—2, *Quartz* prisms—3, *Copper* in cubes, with octahedral modifications, planted on, and moulded to, the quartz crystals.

No. 24. SAME BED.—1, *Prehnite*—2, *Quartz*—3, *Calcite*.

No. 25. SAME BED.—1, *Analcite* in pellucid crystals—2, *Calcite; Orthoclase;* in this specimen the analcite appears to have incrusted some mineral which has disappeared, and the feldspar crystals occur in the cavity thus formed as well as on the outer surface of the analcite.

No. 26. SAME BED.—1, *Prehnite*—2, *Quartz*—3, *Copper*, in threads often moulded to the quartz—4, *Orthoclase* in minute crystals planted on the prehnite, quartz and copper.

No. 27. SAME BED.—1, *Prehnite*, penetrated with copper threads—2, *Quartz* in prisms—3, *Chlorite-like mineral* mentioned in Nos. 20 and 21; here also it has eaten into the faces of the quartz crystals—4, *Analcite* crystals, much fractured and eaten away, and sometimes quite hollow.

No. 28. SAME BED.—1, *Prehnite* in places cavernous—2, *Quartz* in prisms in the cavities in the altered prehnite—3, *Orthoclase* crystals planted on the quartz.

No. 29. SAME BED.—1, *Prehnite*—2, *Copper* traversing the prehnite in the form of threads, etc., ending in crystals which adapt themselves to the crystalline surface of the prehnite.

No. 30. SAME BED.—1, *Prehnite*—2, *Analcite*—3, *Copper*, in flakes on the analcite—4, *Orthoclase; chlorite-like mineral.*

No. 31. SAME BED.—1, *Quartz* in prisms—2, *Orthoclase* crystals, planted on the quartz.

No. 32. SAME BED.—1, *Prehnite*—2, *Copper* in crystals whose under surfaces are moulded to the crystalline surface of the prehnite.

No. 33. HURON MINE.—On the amygdaloid containing smaller amygdules of delessite and quartz, lie:—1, *Laumontite*, a crystalline layer with projecting crystals—2, *Calcite* crystalized upon and wholly enveloping the laumontite crystals.

No. 34. WESTERNMOST ADIT ON THE "SOUTHSIDE." 1. *Analcite*, in opaque crystals $\frac{1}{8}$ to $\frac{1}{3}$ inch in diameter. 2. *Orthoclase* crystals planted on the analcite. (The rock containing this is chocolate-brown, and filled with small amygdules of 1st Laumontite, 2d Calcite).

No. 35. "RAGGED AMYGDALOID." ST. MARY'S. This is a soft brown amygdaloid with brown streak, in which the cavities have assumed the most irregular shapes and merge into each other in a manner which gives to the rock a highly brecciated appearance. The cavities are generally partially open at their wider points; and the minerals occupying them are chiefly the following, often accompanied by a white clay. On the rock lie, 1. *Analcite.* 2. *Orthoclase* crystals on the analcite. 3. *Calcite* over both the foregoing members.

No. 36. SAME BED. On the rock lies *Calcite. Orthoclase* crystals on the calcite.

No. 37. SAME BED. On the rock are scattered, 1. *Analcite* crystals. 2. *Calcite* on the analcite.

No. 38. SAME BED. 1. *Analcite* in large crystals; much altered. 2. *Orthoclase* crystals planted upon the outer surface of, and in cavities in, the analcite.

No. 39. PEWABIC COPPER-BEARING BED. On the amygdaloidal rock lies, 1, *Calcite; copper.* 2. *Datolite* in a granular mass incrusting the calcite crystals.

No. 40. SAME BED? 1. *Calcite* in scalenohedrons. 2. *Datolite*, a granular mass of microscopical crystals. Here the datolite has partially displaced the calcite; only the points of the crystals of the latter were exposed, all the rest being embedded in the younger datolite. These free-standing points remain perfect in glance and form, while wherever the calcite crystals are in contact with the datolite, their surfaces are roughened and perceptibly eaten into. The calcite crystals rest upon a granular mass of the same variety of datolite, which is perhaps the result of a displacement of calcite.

No. 41. "EVERGREEN BLUFF." 1. *Quartz* prism. 2. *Orthoclase* in minute crystals. 3. *Calcite* in simple and twin scalenohedrons.

No. 42. "AMYGDALOID" MINE. (Fissure vein). 1. *Copper.* 2. *Compact datolite.*

No. 43. SAME VEIN. 1. *Prehnite* in its characteristic form. 2. *Copper* conforming to the radiating cleavage-structure of the prehnite. 3. *Datolite*, compact.

No. 44. LOCALITY UNKNOWN. 1. *Prehnite* in its characteristic form. 2. *Quartz* in prisms on the prehnite. 3. *Analcite* crystals on the quartz. 4. *Orthoclase* crystals on the analcite.

No. 45. WESTERNMOST ADIT. "SOUTHSIDE." Small amygdules consist of, 1. *Laumontite.* 2. *Quartz* surrounded by the laumontite.

No. 46. MICHIGAN MINE (Fissure vein). On the veinstone, which is here a greenish-gray, hardened clay-like material with flakes of chlorite and copper, and which becomes brown and soft near the contact with No. 1, lies, 1. *Datolite* in a uniformly distributed layer $\frac{1}{10}$ of a line to 2 lines thick, with the free surface highly crystallized. The crystals are transparent and rose-colored from the presence of minute particles of copper. The datolite appears quite fresh, and the copper seems to be confined to it. 2. *Calcite*, four small slightly yellowish semi-transparent rhombohedrons, modified with steeper scalenohedron faces, lie upon the datolite. 3. *Orthoclase*, yellowish crystals, $\frac{1}{20}$ of an inch long, are scattered over the surface of the specimen, some lying upon the calcite and some upon the datolite.

No. 47. MANY LOCALITIES. 1. *Prehnite.* 2. *Delessite.* The prehnite which occurs as the solid filling of amygdaloidal cavities in the upper part of many beds, is subject to alteration to chlorite. It is very common to see the prehnite soft and green to a slight depth from the outer surface of the amygdule, without any line of separation between this portion and the hard center; and in the interior the prehnite often passes gradually into spots and flakes of chlorite. In these amygdules the prehnite is characterized by a radiating structure, starting from

a single center. It is along these planes of radiation that the change begins. Every possible gradation is observable. The resulting product is sometimes a mass of foliated chlorite, but more generally it is an amygdule of compact chlorite, exhibiting in its fracture the same radiating structure as the prehnite.

No. 48. SHELDEN AND COLUMBIAN LOCATION. 1. *Prehnite* which is the general filling of the cavities in the upper part of the amygdaloid of this locality. 2. *Feldspar*, red. It is quite an exceptional occurrence in this neighborhood, and it is intimately associated with the prehnite in a manner that makes it seem to be pseudomorphous after it.

Crystals of epidote and of quartz occur on this feldspar, but the specimen gives no insight into their relation, as regard age, either to each other or to the feldspar as a secondary product.

No. 49. SOUTH PEWABIC MINE. In this bed a frequent form of the rock is a compact amygdaloid, of which 50 or 60 per cent of the volume consists of amygdules from the size of a pin-head to $\frac{1}{8}$ inch in diameter. The matrix in a specimen before me is brown, and too hard to be scratched with a knife. The amygdules are sometimes of calcite, but more generally contain, 1. *Quartz*, clear and filling the cavity. 2. *Chlorite; copper.* The chlorite (apparently delessite) appears to displace the quartz; in some amygdules it merely penetrates the fissures of the quartz, giving to this a green color; in others nothing remains but a cavernous mass of quartz and chlorite usually well charged with copper; indeed the copper occurs here only with this chlorite.

The series to which this bed belongs is represented on the "Southside," and again on the St. Mary's, by amygdaloids which resemble this one in all essential particulars, except that the amygdules are there filled chiefly with laumontite, and are free from copper and nearly free from chlorite.

No. 50. "OSSIPEE AMYGDALOID." The rock is compact, spotted green and brown, and contains small *separate* amygdules of prehnite, quartz, epidote, calcite, chlorite. The chlorite appears as a destroyer of prehnite, quartz and calcite. A larger cavity shows the following succession: 1. *Prehnite;* a lining $\frac{1}{8}$ to $\frac{1}{8}$ inch thick, much altered and in places changed to chlorite. 2. *Orthoclase*, minute crystal. 3. *Epidote* on the feldspar.

No. 51. HURON MINE. In places, the Isle Royale copper-bearing bed consists largely of a light grayish-green, fine-grained rock of epidote and chlorite indurated with quartz; small, irregular cavities in this contain, 1. *Epidote;* crystalline lining $\frac{1}{4}$ inch thick. 2. *Quartz* filling the interior.

No. 52. SHELDEN AND COLUMBIAN MINE. In cavities in a brown and green amygdaloid lie, 1. *Quartz* in well shaped prisms. 2. *Calcite; Quartz.* This second quartz is in small and very much distorted crystals, which are often partially imbedded in the calcite, and are also often planted on the older quartz, from which they can be easily removed without fracture.

No. 53. HURON MINE. 1. *Quartz* with more or less crystalline structure. 2. *Copper* moulded on the quartz and filling cracks and interstices in it.

No. 54. RAGGED AMYGDALOID. ST. MARY'S. In the rock (see No. 35), some of the smaller cavities contain, 1. *Orthoclase* as a thin crystalline lining. 2. *Calcite* filling the interior. 3. *Chlorite-like mineral* penetrating and apparently replacing the calcite.

In a larger cavity occur the following:

No. 55. 1. *Analcite* crystals ¼ inch to 2 inches in diameter, much reddened. 2. *Orthoclase;* small crystals on the analcite. 3. *Clay?* a soft white mineral, apparently the result of continued decomposition of the analcite under conditions unfavorable for the formation of new silicates as feldspar.

No. 56. "ANCIENT PIT" BED. DOUGLASS LOCATION. 1. *Epidote* forming a crystalline lining of a cavity. 2. *Quartz* filling the interior.

No. 57. SULPHURET (Fissure vein) HURON LOCATION. The vein (6–8 inches wide) consists of the following: 1. *Ankerite?* (crystalline, massive, white on fresh surface, but rusty-brown on exposed fractures) forming the member nearest the wall on each side. 2. *Quartz* in two symmetrical comby bands on the dolomite and in thin seams in the dolomite connected by cross-seams with the quartz-comb. 3. *Chalcocite*, black—bluish-black *with distinct cleavage.* It resembles the pseudomorphous chalcocite of the Lac la Belle mine. Bornite occurs sprinkled through the chalcocite in minute specks; in places it predominates, sometimes to the exclusion of the latter.

These sulphurets form the central member, and bunches of them are often surrounded by the older members, giving the "cockade" structure to the vein.

Note.—It is in another portion of this vein that the arseniurets, whitneyite and domeykite are found.

I have attempted to bring the foregoing observations into the following tabular form for greater convenience of comparison. The table is unavoidably imperfect, owing to the necessary difficulties which arise in attempting to compare the successions of different localities. The detailed observations, however, will serve for a check upon the imperfections of the schedule.

COPPER AND CALCITE.—In many of the instances in which calcite crystals are found enclosing copper, it is difficult and

Capen Vein,	1	Laumontite		Prehnite				Chlorite	Analcite	Calcite					
Huron Mine,	2	Laumontite		Prehnite											
Copper Falls Mine,*	3	Laumontite							Analcite						
" " " *	4								Apophyllite		Copper		Orthoclase		
Bay State Mine,*	5			Prehnite	Quartz		Copper								
Phœnix Mine,*	6	Laumontite			Quartz			Green-earth.							
Bay State Mine,*	7				Quartz				Apophyllite	Calcite					
Bohemian Mine,*	8								Analcite		Copper		Orthoclase		
Amygdaloid Mine,	9			Prehnite	Quartz				Analcite				Orthoclase		
Bay State Mine,	10								Analcite				Orthoclase		
Amygdaloid Mine,	11			Prehnite,Cu								Datolite			
" "	12		Calcite	Prehnite,Cu						Calcite.		Datolite			
Pewabic Lode,	13	Laumontite			Quartz		Copper	Chlorite or Green-earth.		Copper.		Datolite			
" "	14	Laumontite			Quartz	Calcite	Copper	Chlorite or Green-earth.		Calcite			Orthoclase		
Alb. and Bost. "Ragged Amygd."	15			Prehnite									Orthoclase		
"Epidote Lode," St. Mary's,	16			Prehnite	Quartz			Chlorite or Green-earth.	Analcite						
Amygd. Kearsarge,	17			Prehnite	Quartz										
Huron Mine.	18								Analcite	Calcite					
Alb. & Bost. Amygd.	19			Prehnite	Quartz								Orthoclase		
" " "	20			Prehnite	Quartz			Chlorite or Green-earth.					Orthoclase	Calcite	
" " "	21				Quartz			Chlorite or Green-earth.						Calcite	
" " "	22			Prehnite	Quartz								Orthoclase		
" " "	23			Prehnite	Quartz, Cu										
" " "	24			Prehnite	Quartz									Calcite	
" " "	25								Analcite				Calcite.		
" " "	26			Prehnite	Quartz, Cu								Orthoclase. Orthoclase		
" " "	27			Prehnite	Quartz			Chlorite or Green-earth.	Analcite						
" " "	28			Prehnite	Quartz								Orthoclase		
" " "	29			Prehnite,Cu											

Alb. & Bost. Amygd.	30	------	-----	Prehnite	-----	---	-----	-------	Analcite	---	Copper	----	Green-earth or Chlorite. Orthoclase.	----
" " "	31	------	-----	------	Quartz	---	-----	-------	------	---	----	----	Orthoclase	----
" " "	32	------	-----	Prehnite,Cu	-----	---	-----	-------	------	---	----	----	------	----
Huron Mine,	33	Laumontite	Calcite	------	-----	---	-----	-------	------	---	----	----	------	----
"Southside,"	34	------	-----	------	-----	---	-----	-------	Analcite	---	----	----	Orthoclase	----
"	"	Laumontite	Calcite	------	-----	---	-----	-------	------	---	----	----	------	----
"Ragged Amygdaloid,"	35	------	-----	------	-----	---	-----	-------	Analcite	---	----	----	Orthoclase	Calcite
" "	36	------	-----	------	-----	---	-----	-------	------	Calcite	----	----	Orthoclase	----
" "	37	------	-----	------	-----	---	-----	-------	Analcite	Calcite	----	----	------	----
" "	38	------	-----	------	-----	---	-----	-------	Analcite	---	----	----	Orthoclase	----
Pewabic Bed,	39	------	-----	------	-----	---	-----	-------	------	Calcite	Copper	Datolite	------	----
" "	40	------	-----	------	-----	---	-----	-------	------	Calcite	----	Datolite	Orthoclase	Calcite
Evergreen Bluff,	41	------	-----	------	Quartz	---	-----	-------	------	---	Copper	Datolite	------	----
Amygdaloid Mine,	42	------	-----	------	-----	---	-----	-------	------	---	Copper	Datolite	------	----
" "	43	------	-----	Prehnite	-----	---	-----	-------	------	---	----	----	Orthoclase	----
Unknown locality,	44	------	-----	Prehnite	Quartz	---	-----	-------	Analcite	---	----	----	------	----
"Southside,"	45	Laumontite	-----	------	Quartz	---	-----	-------	------	---	----	----	Orthoclase	----
Michigan Mine,	46	------	-----	------	-----	---	-----	-------	------	---	Datolite	Calcite	Orthoclase	----
Many localities,	47	------	-----	Prehnite	-----	Delessite		-------	------	---	----	----	------	----
Shelden & Columbian,	48	------	-----	Prehnite	-----	---	-----	-------	------	---	----	----	Feldspar	----
South Pewabic Mine,	49	------	-----	------	Quartz	---	-----	Chlorite or Green-earth. Copper.	------	---	----	----	------	----
Ossipee Amygdaloid,	50	------	-----	Prehnite	-----	---	-----	-------	------	---	----	----	Orthoclase	Epidote
Huron Mine,	51	------	Epidote	------	Quartz	---	-----	-------	------	---	----	----	------	----
Shelden & Columbian,	52	------	-----	------	Quartz	Calcite. Quartz.	-----	-------	------	---	----	----	------	----
Huron Mine,	53	------	-----	------	Quartz	---	Copper	-------	------	---	----	----	------	----
Ragged Amygdaloid,	54	------	-----	------	-----	---	-----	-------	------	---	----	----	Orthoclase	Calcite Chlorite
" "	55	------	-----	------	-----	---	-----	-------	Analcite	---	----	----	------	Clay
"Ancient Pit" Bed, Douglass,	56	------	Epidote	------	Quartz	---	-----	-------	------	---	----	----	Orthoclase	----
'Sulphuret V'n,' Huron,	57	------	Ankerite ?	------	Quartz	---	Bornite. Chalcocite	-------	------	---	----	----	------	----

often impossible to distinguish as to the relative ages of the two. But specimens in my collection offer conclusive proof that each of the following cases occur.

I.—*The copper was present before the calcite began to form, and became enclosed in the growing crystal.*

In this case the copper and its associated minerals generally form the basis on which the calcite rests, and the crystals of the latter exhibit entering faces wherever the surface of the crystal is in contact with the copper; it should seem to indicate an effort at those points to crystallize free from the foreign substance, by forming separate individuals. But on the finished crystal the traces of this tendency is visible, generally, only in the comparatively very small entering faces at the contact with the copper.

In this way calcite crystals, formed in a cavernous mass of copper, are intersected internally by a perfect net-work of thin plates of the metal, and yet preserve their cleavage unaffected; but wherever the copper comes in contact with the surface of the crystal, the small entering faces are present.

II.—*The crystal of calcite was partly formed, then became incrusted with copper, and was finished by a new growth of calcite over the metallic film.*

A most remarkable instance of this case is that of a crystal about 2 inches long—a steep scalenohedron—with a basal termination of about 1 square inch surface. At this stage of its growth it was covered, over nearly the whole surface, with a thin coating of copper. The basal termination on scalenohedrons of calcite is as rare on Lake Superior as elsewhere, and in the few instances where I have seen it, it lacks the polish which indicates perfect growth. The tendency to complete the point of the scalenohedron is well shown on this specimen; over the partially copper-coated basal plane there are scattered a large number of perfectly pointed scalenohedrons—two or three of these are $\frac{1}{8}$ to $\frac{1}{2}$ inch high—and others are scattered over the side-faces. All of these younger crystals are arranged in perfect uniformity with the plan of the underlying, older individual.

Those portions of the surface on which the copper-coating is perfect have no younger calcite crystals; these occur where the metallic film is thinnest and more or less perforated.

The copper is not confined absolutely to the surface of the crystal on which it lies; it penetrates to a slight distance along the cleavage-planes, and the result is an exceedingly delicate treiculation on its under surface. The calcites which are planted on the copper contain brilliant particles of the me tals

swimming, if one may use the word, in the interior of the crystals; and these are so disposed as to lead to the idea that throughout the growth of the younger crystals they had to contend with the continued deposition of the metal. Thus one of the new scalenohedrons, after growing to the height of $\frac{1}{4}$ inch, was, like the underlying one, also ended with a basal termination, on which again smaller new and well pointed individuals were built up.

III.—*The copper has entered the calcite crystal since its growth was finished.*

A specimen, in my collection, illustrates this remarkably well. It is a cleavage-rhombohedron of opaque calcite, traversed by intersecting sheets of copper, which are wholly independent of the cleavage planes. On detaching the copper from the calcite, the surface of the latter appears rough; it is a fracture oblique to the cleavage, and the face of the fracture is formed by countless corners, or solid angles, of minute cleavage-rhombohedrons, as is fully proved by the reflexion of the light. The copper-sheets, which are about $\frac{1}{40}$ inch thick, reproduce this very completely.

Another very remarkable specimen is from the cement of the Albany and Boston conglomerate. It is about 1 inch in diameter, and consists of opaque white calcite. The continuity of the cleavage shows it to be a single individual, though it passes on the edges without any sharp demarkation into the common cement of the conglomerate. This calcite is traversed by continuous sheets of copper $\frac{1}{250}$ to $\frac{1}{40}$ inch thick, which are perfectly straight. These sheets are parallel to several planes, (nearly all of which are independent of the cleavage) and intersect each other. In each of the sets thus formed the sheets are perfectly parallel, and are separated by plates of calcite, which are in places as thin as the copper itself. Where three such sets intersect each other, the resulting solid appears composed of concentrically arranged laminæ of copper and calcite. In some parts of the specimen the copper predominated over the calcite. Wherever the faces of the copper laminæ are exposed, they are marked with a delicate, reticulated tracery, indicating the lines of intersection of the sheet with the cleavage planes of the crystal. The cement in the vicinity of the calcite is impregnated with copper; in places it is almost wholly replaced by the metal in the fine granular condition called "brick copper," and into this the laminæ of metal extend, without break, from the calcite. This specimen is really a pseudomorph of copper after calcite.

Copper and Silver.—It is a well known fact that these two metals occur in the metallic state, in the Lake Superior depos-

its, in the most intimate contact with each other, and yet without being mutually alloyed. Even at the contact they are not absolutely joined together, for after rolling out a piece of copper containing spots of silver, the two metals become more or less separated, and may often be readily detached from each other. I have not been able to obtain any material that would throw light upon the relative ages of the two metals.

[To be continued.]

ART. XXXIV.—*On Photographing Histological Preparations by Sunlight;* by J. J. WOODWARD, Asst. Surgeon U. S. Army. Report to Surgeon General J. K. Barnes, U. S. Army. Washington, June 9, 1871.

IN January, 1870, I had the honor to submit to you a report in which I detailed the results of a series of experiments, which showed the superiority of the electric and magnesium lights over sunlight, as heretofore employed, for the production of photo-micrographs of the soft tissues. In June of the same year I made a report in which I showed that similar results could be obtained with the oxy-calcium light. With these various artificial sources of light, I obtained pictures which appeared to me to be "clearer and better defined than any photographs of similar objects I had hitherto seen produced by sunlight."

So many cloudless days are offered to the photographer in Washington, that I could not but regret these results; yet they appeared to be final at the time of writing. During the last few months, however, I have found improved methods of using the light of the sun for photographing the soft tissues, and have arrived at results which must materially modify the conclusions of my former reports.

Not that I have anything to withdraw from the opinions I have expressed, as to the certainty and success attending the use of artificial lights for the purpose named, but I have much to add with regard to the most advantageous methods of using the light of the sun for obtaining satisfactory pictures of tissue preparations, and such other objects as approximate them in optical characteristics.

If a well made preparation of some normal tissue, or of some pathological growth, stained with carmine, silver, or gold, and mounted temporarily in glycerine, or permanently in Canada balsam, be illuminated by white cloud illumination, or by lamp light, and found to be all that could be desired, it will nevertheless appear very unsatisfactory if illuminated by the direct rays of the sun.

The eye glancing through the tube of the instrument, dazzled by the powerful light, discerns amidst the blaze, innumerable colored rings, produced by diffraction and interference which disturb the normal appearances of the preparation and render its interpretation impossible.

If the image be received upon a white screen similar phenomena obtrude themselves, destroying the clearness of the picture, though no longer injuriously affecting the eye; and if monochromatic light is employed, although the disorderly play of color disappears, black rings and lines of the most manifold character and direction take their place. Pictures produced under these circumstances are of course quite useless, and the difficulty occurs not merely in the case of tissue preparations, but in a very large number of other objects.

To escape these disagreeable results, it has heretofore been the practice to pass the solar pencil through a piece of ground glass. This plan is recommended in all the treatises on photo-micrography, and has hitherto been employed in the solar work done at the Army Medical Museum. The method is effectual in getting rid of the diffraction and interference phenomena complained of; an image is obtained which is clear and satisfactory to the eye looking down the tube, but it appears very weak on the screen, and is sadly deficient in contrast. These faults are reproduced in photographs of objects thus illuminated, and, moreover, the time of exposure is enormously increased. Such pictures are decidedly inferior to those which can be obtained by the magnesium, or even by the calcium light, with which no ground glass is used.

I desire now to call your attention to the fact that in the course of some recent experiments, I have ascertained that the diffraction and interference phenomena above complained of, may be prevented by the use of a suitable condensing lens, even better than by the ground glass; that by this plan the exposure may be greatly diminished, say from three minutes for five hundred diameters, to a fraction of a second, and that the resulting pictures are not merely quite as free from diffraction and interference phenomena as the best that can be obtained when the ground glass is used, but are characterized by greater contrast and superior sharpness of definition.

The details of my new method are as follow: The microscope being placed on a shelf at the window of the dark room, and its body made horizontal, the achromatic condenser is illuminated by a solar pencil reflected from a heliostat upon a moveable mirror outside the shutter and thence into the dark room, precisely as described in my original paper on photo-micrography.*

* This Journ., II, vol. xlii, p. 189, Sept., 1866.

No ground glass is used, but instead a lens mounted in a suitable tube is fixed in the opening of the shutter through which the solar pencil enters. This lens is an achromatic combination about two inches in transverse diameter and of about ten inches focal length. It is placed at such a distance from the achromatic condenser that the solar rays are brought to a focus and begin again to diverge before they reach the lowest glass of the achromatic condenser.

For anatomical preparations requiring for their display from two to five hundred diameters, I use an $\frac{1}{4}$th of an inch objective, without an eyepiece, obtaining the precise power desired by variations in the distance of the sensitive plate from the stage of the instrument. I have lately given the preference to immersion objectives, the corrections of which I find are generally well suited to photographic requirements.

Now with an 1-8th objective and the arrangement above described, the field is so brilliantly illuminated that the eye cannot safely be permitted to look down the tube. The image is therefore received on a piece of white card-board, and sitting by the microscope to make the adjustment, I view the card with both eyes precisely as in the case of the ordinary solar microscope. With these arrangements, the card-board placed from two to four feet from the stage of the microscope is sufficiently well illuminated to permit distinct vision, even when objectives of the shortest focus are used and powers of five to ten thousand diameters obtained. While the object is thus seen on the white screen in its natural colors, the cover corrections, focussing, management of the achromatic condenser, and selection of the portion of the preparation to be photographed, are readily managed. When all is satisfactory, I insert an ammonio-sulphate cell between the large lens and the achromatic condenser, and draw down the velvet hood which prevents leakage of light from about the microscope into the dark room; then going to the plate holder, I make the final focussing in the usual way on the ground glass, or on plate glass with the help of a focussing glass, according to the nature of the object.

With powers of five hundred diameters or less, I at first experienced some difficulty in giving the right exposure; for as the time required was but a fraction of a second, it was a matter of some difficulty to regulate it with precision. At length I succeeded by arranging a sliding shutter, with a transverse slit of variable width, so adjusted as to fall with its own weight before the tube of the microscope, the exposure being made during the passage, and the time of exposure regulated by the width given to the slit.

Of course it occurred to me that for such short exposures the heliostat might be dispensed with, and I found on trial without

it that a large right-angled prism used in the position of total reflection, or even an ordinary mirror gave excellent results; the exposures being even shorter than when the heliostat was used, since there was but a single reflection. I could not satisfy myself, however, that the quality of the pictures differed from those obtained with the help of the heliostat, except perhaps that in certain cases the prism seemed to offer advantages which will be referred to hereafter. Under these circumstances the heliostat appears desirable for ordinary use, since the solar pencil being thrown in a constant direction, the trouble of adjusting the illumination of a series of objects is considerably diminished; but I have convinced myself by trial that equally good pictures can be produced without it, even with very high powers, a circumstance of considerable interest where motives of economy preclude the microscopist from procuring this convenient instrument

A few remarks with regard to certain points in the procedure above sketched seem called for.

First, with regard to the selection of objectives suitable for photographic work of this kind. The power of the objective to be used will depend of course upon the details it is desired to display. I find it best to use the naked objective without eye-piece or amplifier, and not as a rule to fix the sensitive plate more than three or four feet from the stage of the microscope. An 1-8th objective may be conveniently employed to obtain powers of from two to five or six hundred diameters, a 1-16th for higher powers up to twelve or fifteen hundred diameters. Suitable amplifiers or even eye-pieces may be used in either case, with great increase of the magnifying power, and often with admirable scenic effect, but there is always a certain loss of definition. Still such amplifications may sometimes be advantageously resorted to, especially in the case of objects which present very minute details; for in these cases the paper prints will often lose many of the fine details of the negative, and the loss of definition incurred by the amplifier or eye-piece is not unfrequently less than that encountered in attempting to transfer to paper a negative prepared with insufficient magnifying power. Thus far my experience is decidedly in favor of using sufficient power in the first instance, rather than attempting to enlarge negatives taken with less power.

The objective selected should of course be unexceptional in defining power, and should always be specially corrected for photography. It has been erroneously stated by Moitessier,* that if an ammonio-sulphate or other blue cell be interposed in the solar pencil, all special corrections of the objective may be

* La Photographie Appliquée aux Recherches Micrographiques, par A. Moitessier, Paris, 1866, p. 180 et seq.

dispensed with. This proposition, which has been adopted by many other writers, appears plausible, but a little consideration will show it to be quite erroneous.

Every one knows that a good objective must be free from spherical, as well as from chromatic, aberration. Of course the use of monochromatic light disposes of the chromatic trouble. Not so with the spherical aberration. Now this aberration, like the chromatic, is corrected mainly by the just combination of flint with crown glass in the several pairs which constitute the objective. If these are so adjusted as to correct spherical aberration as nearly as possible for white light, they will no longer do so for light which has passed through the ammonio-sulphate of copper. Until objective makers take this fairly into consideration, the microscopist who desires to photograph what he sees is left to a happy chance in the selection of his objectives. For even those makers who profess to prepare objectives "specially corrected for photography" do not deal any too well with the problem. If they would test their objectives, while making them, with violet light, we should have better results; for with such illumination the eye can see all that photography can execute, and no more.

But this circumstance fortunately enables the microscopist to select from the objectives in the market those which are suitable for photography. It is only necessary to test their performance when illuminated by sunlight, which has passed through an ammonio-sulphate cell. Now it fortunately happens that the high power immersion objectives of certain makers, especially those of Powell & Lealand, possess very nearly the corrections which theory would indicate as best adapted for photographic use. Nevertheless it can hardly be doubted that even these objectives could be greatly improved if the makers would take into consideration the principles involved in the foregoing remarks.

A second point, which deserves attention, is the use of the large condensing lens above described. This lens, it will be understood, corresponds with the large condensing lens of the ordinary solar microscope, while the achromatic condenser takes the place of the so-called field glass of the same instrument. It has already been mentioned that this lens should be placed at such a distance from the achromatic condenser that the solar rays may be brought to a focus, and begin again to diverge before they reach its lowest glass. A different arrangement is usually employed in the solar microscope, the field glass being placed at such a distance from the first condenser that the solar rays impinge upon it before they come to a focus. As a consequence, the convergent pencils proceeding from the first lens are still further converged by the field glass, and a burning

focus of heat, as well as of light, is produced, which is damaging to the preparation as well as to the balsam cement of the objectives used. If, however, the rays from the first lens are permitted to come to a focus and to begin to diverge before striking the second, this latter can readily be adjusted so as to bring the illuminating rays to a handsome focus, while the heat rays, after passing the second lens, become parallel or even divergent according to the position of the achromatic condenser, and all trouble from the solar heat is thus completely avoided. So successfully may this separation be effected, indeed, that I have frequently obtained light enough to give distinct vision and admirable definition on the card-board screen with five thousand linear diameters or even higher powers (obtained by the immersion 1-16th, an amplifier, and four feet or greater distance), while the heat was so slight that the drop of water used with the immersion lens did not require renewal oftener than about once in two hours.

I had employed this device for several months, and supposed it to be quite novel, when I read the paper of the late distinguished President of the Royal Microscopical Society of London, the Rev. J. B. Reade, "On the separation of the rays of heat from the rays of light in solar and oxy-hydrogen-gas microscopes."* I learned from that article that Mr. Reade had devised this very plan as an improvement to the solar microscope as long ago as 1836. The advantages attained may be stated in his own lucid words.

"It is evident by this arrangement of lenses we convert the parallel solar beam first of all into a cone of light-giving rays within a cone of heat-giving rays, and the principal focus of heat is farther from the condensing lens than the principal focus of light. But after these rays cross the axis we have, conversely, an equal and opposite cone of heat-giving rays within a cone of light-giving rays, and a plano-convex lens or hemisphere, if placed in this second cone at the distance of its own focal length from the principal focus of heat, will be at a distance greater than its focal length from the principal focus of light; and, consequently, the rays of heat, after passing through this lens, will become parallel, while the rays of light converge to a second focus. I have approximately measured the heating power of the thermal rays of the second cone when rendered parallel by the plano-convex lens, and I found, in the month of December, that the mercury in a sensitive thermometer, when placed in the second focus, did not reach 90° Fah., while at the same time the heat at the focus of the first cone was sufficient to discharge gunpowder."

* The British Journal of Photography, Dec. 16, 1870, p. 590.

Mr. Reade appears to have experimented with low power objectives only, for he speaks merely of such preparations as the head of a flea. He therefore succeeded very well by using a single lens in his field glass. With such powers as the immersion $\frac{1}{8}$th and $\frac{1}{16}$th, I find it better to use an ordinary achromatic condenser instead. The principles involved are of course identical. For the first condenser also, I have been using an achromatic combination of the dimensions and focal length above mentioned, taken from the back of an ordinary photographic protrait tube; but I am not sure that a simple plano-convex lens of the requisite diameter and focal length would not answer every purpose.

The introduction of the ammonio-sulphate cell would of itself prevent the passage of most of the heat rays falling upon it, but if this were the only means of excluding them, it would not be possible to focus primarily with white light on the card-board screen in the manner which I have found so convenient.

I have already stated that the time of exposure required for the production of pictures magnified five hundred diameters or less, was a fraction of a second. With higher powers it increases, varying with the management of the achromatic condenser. For four thousand diameters I have sometimes needed as much as twenty-five seconds.

So long as the exposure is greater than a second, the requisite time may readily be given with a piece of velvet, or a card-board screen held in the hand. For shorter exposures some mechanical contrivance is indispensable. That alluded to above seems to answer every purpose, and is arranged as follows: A wooden screen is fixed between the microscope and the sensitive plate, as close as convenient to the microscope. To prevent side lights reaching the plate, the screen is connected with the window shutter by velvet curtains, which can be turned aside to manipulate the instrument, and be let down at the proper time. A circular hole, three inches in diameter, is made in the screen opposite the tube of the microscope for the transmission of the image. In front of this a light shutter slides loosely up and down, held in place by a cleat of wood on each side, the design being to permit the shutter to fall edge foremost with as little friction as possible. The shutter may be made of thin metal, of wood, or even of card-board. I am using one of pine wood the $\frac{1}{16}$th of an inch thick—I have used one of card-board with equal success. In the shutter is an opening, three inches wide by ten long, covered with a card-board slide, by means of which any width of slit, from a fraction of an inch to ten inches, can be given. The part of the shutter below the slit closes the aperture through which the image passes when the shutter is fixed in place before the exposure is made. On

drawing a wooden trigger the shutter is started on its fall, which is arrested by a piece of string of suitable length. The exposure has now been made, but the aperture through which the image passes is again closed, this time by the part of the shutter above the slit. The shutter is so light that the jar caused by the sudden arrest of its motion by the string is too trifling to do any damage to the microscopic apparatus, and as it occurs after the exposure is over it cannot affect the image. I find that if, when the shutter is started, the lower edge of the slit is an inch above the aperture through which the image passes, a convenient velocity is attained for a magnifying power of two to five hundred diameters, arranged as I have described. For still shorter exposures, necessitated by lower powers or other circumstances, it would be best to start the shutter from a greater height, which would give greater velocity to the passage of the slit, and any available fraction of time desired might thus conveniently be obtained. The whole arrangement is inexpensive, and may be manufactured in a few hours by any one, out of a deal board, a few pieces of card-board and a yard or two of cotton velvet.

Of course the fractional measures of time obtained in this way are not absolute, since the friction must be variable, unless the apparatus were made in a more costly manner of metal. But I have found that the variations thus introduced are so small that they may be disregarded, and that while the starting point remains the same, the width of the slit in the falling shutter indicates fractions of time which may confidently be counted upon to give proportional photographic results.

The next subject for remark is the arrangement employed when the heliostat is dispensed with.

For this purpose the contrivance usually employed for the solar microscope answers very well. A circular disc of brass, with toothed edges, is let into a square plate of the same metal, and is turned by a small toothed wheel, to which a suitable button or milled head is attached. Through the center of the disc passes a tube six or eight inches long and two inches in diameter, the outer extremity of which is fitted to receive the large condensing lens. Just below this tube an arm is firmly attached to the outer surface of the disc for the purpose of carrying the mirror or right-angled prism, to which any desired inclination can be given by a rod passing through the disc by the side of the tube. The whole arrangement is quite like the similar parts of the ordinary solar microscope, and hence needs no minute description: it is fitted into a window shutter, which must of course face to the south, and the room being darkened, the motions of the mirror or prism can readily be controlled

from within. If the condensing lens is used, I do not think any material advantage can be obtained from the prism, and its expense is a decided objection. In the winter season, in this latitude, a prism of over five inches hypothenuse is required, and its cost is a serious item. An ordinary glass mirror answers, I think, quite as well for the tissues and most other purposes. There are, however, certain objects, such as the Pleurosigmata and some other diatoms, the Nobert's test-plate, and the scales of certain insects, for which the condensing lens is unnecessary. The achromatic condenser, illuminated by a parallel solar pencil, answers better in these cases, and if it is properly managed no diffraction or interference phenomena are produced. I am satisfied that in such cases the pure parallel pencil obtained from the prism gives better definition to the image than can be obtained by the double pencil reflected from an ordinary glass mirror. A mirror silvered on the reflecting surface would, I suppose, answer the same purpose; but such mirrors are not permanent, and are troublesome to keep in order while they last. Moreover, if the prism is used only for this purpose, a very small and cheap one will answer, since a pencil half an inch in diameter is all that is required. Such a small right-angled prism is furnished with most large microscopes, and can readily be mounted outside the brass disc so as to answer the special purpose indicated. For all those objects which require the large condensing lens to avoid diffraction and interference, a common glass mirror will answer well enough. For lower powers than two hundred diameters, however, the ordinary mirror will often be found to reflect too much light, and the image on the card-board screen will be found too brilliant to be conveniently observed for any length of time. In such cases a piece of plain unsilvered plate glass may be substituted for the mirror. The greater portion of the solar light passes through it and is lost, but enough is reflected to make pictures of four hundred diameters in from two to three seconds exposure, and these pictures have all the qualities of those made with ordinary mirrors. I have tried instead to diminish the light by absorbing a part, using for this purpose an ammonio-sulphate cell of considerable thickness, but find that this plan diminishes the contrast and definition of the image, which is not the case when a mirror of simple plate glass is used as above described.

With regard to the management of the plate-holder, the apparatus for focussing, and other accessory arrangements, I need only say that I employ for the solar light the same simple plan which I have described in full in my reports on the use of artificial lights in photo-micrography.

Since making the experiments which have led to the foregoing results, I have modified my method of dealing with the

electric light in photographing the tissues. I first render the divergent pencil proceeding from the carbon points as nearly parallel as possible by means of the condenser, usually supplied with electric lamps for this purpose, and then introduce into the parallel pencil, instead of a ground glass, the very same condensing lens described above for the process with solar light. The image is received primarily on a card-board screen, and the remaining details do not differ from what has been related above. The time of exposure does not exceed a single second for four hundred diameters, and the sharpness of the pictures exceeds any of my former results. Indeed, with this new arrangement, I must say that the electric light appears to me to retain the apparent superiority over sunlight, remarked in my paper on the use of this method of illumination in photo-micrography, at least in the case of all those objects which in themselves possess but little contrast. For well made tissue preparations, however, I find the best work I can do with the electric light so similar to the best attainable by sunlight, used as above described, that I should rarely take the trouble to set up the battery and work the electric lamp, unless it was desirable to work at night or in unfavorable weather.

[The memoir is accompained by photographic plates of tissues, magnified 400 to 500 diameters, as examples of the results obtained in the manner described, and concludes with description of the preparations.]

ART. XXXV.—*Barometrical Measurements in Ecuador;* by W. REISS and A. STÜBEL. Translated from the Spanish by Professor ORTON, Vassar College.

[THE following is an abstract of the principal altitudes near Quito, as determined by the able North German Expedition in 1870–1. Schmidt's value of the *vara*, given below, differs from the standard in the U. S. Office of Weights and Measures by −·0015 meter. I have reduced the meters to English feet. It is remarkable that every successive measurement of the Quitonian Andes gives a reduced elevation. Thus: altitude of Quito by Humboldt, 9570; Orton, 9520; Reiss and Stübel, 9350;—of Panecillo by Humboldt, 10,244; Orton, 10,101; R. and S., 10,006;—of Pichincha by Humboldt, 15,922; Orton, 15,827; R. and S., 15,704;—of the crater by Wisse and Moreno, 13,600; Orton, 13,300; R. and S., 13,175.]

NOTE.—The altitudes are calculated in meters above the level of the sea, one meter equaling 1·1963 Spanish vara. The greater part of the observations were made with the barometer; but trig

onometrical measurements were taken of some notable points. The letters B and T indicate these different means, and the numbers show the times of observations. As this work is provisional, the authors reserve the privilege of making corrections in the future, which, however, will be insignificant.

Place.	Altitude. Meters.	Feet.	Method.
Tulcan, plaza (northern frontier),	2,977	9,767	1 B.
Chota bridge,	1,532	5,026	1 "
Isambal, foot of Yanaurcu,	4,041	13,257	3 "
Top of Yanaurcu,	4,556	14,947	2 "
El Fuyafuya, north summit,	4,294	14,087	2 "
" south "	4,279	14,038	T.
Caricocha,	3,711	12,174	"
Tabacundo,	2,889	9,478	1 B.
Salinas, plaza,	1,639	5,377	3 "
Hatuntaqui, plaza,	2,407	7,897	2 "
Cotacachi, plaza,	2,453	8,048	24 "
Otovalo, plaza,	2,581	8,468	1 "
Hacienda of Cuicocha,	2,747	9,012	2 "
Border of the Lake Cuicocha,	3,118	10,229	T.
Top of Cotacachi, S.E. point,	4,960	16,272	"
" " N.W. "	4,966	16,291	"
" " N. "	4,829	15,842	"
Snow-limit on Cotacachi, S.W. side,	4,620	15,157	"
" " " E. "	4,694	15,399	"
San Pablo, plaza,	2,726	8,943	3 B.
" " lake,	2,697	8,848	2 "
Top of Cusin,	4,012	13,162	2 "
La Esperanza, plaza,	2,344	7,690	16 "
Top of Curilche,	3,882	12,735	2 "
Lake within the crater,	3,801	12,470	2 "
Top of Cerro Cunru,	3,338	10,950	2 "
Lake in the crater,	3,317	10,882	1 "
Yaguarcocha,	2,253	7,391	4 "
Ibarra, plaza,	2,225	7,300	4 "
Imanta, "	2,422	7,946	2 "
Peguche, hacienda,	2,556	8,385	3 "
Guaillabamba, pueblo,	2,106	6,909	3 "
Alchipichi bridge,	1,719	5,639	2 "
Pomasqui, plaza,	2,507	8,225	2 "
Cotocollao, "	2,802	9,193	4 "
Perucho, "	1,830	6,004	3 "
Colicalí, pueblo,	2,792	9,160	2 "
Frutillas,	3,133	10,278	2 "
Mindo, hacienda of San Vicente,	1,264	4,147	16 "
El Pondoña, N.E. summit,	2,940	9,645	1 "
Pailon,	2,985	9,793	1 "
Junction of Rio Blanco with Rio del Volcan,	2,078	6,817	7 "
Hacienda of San José in Lloa,	3,091	10,140	26 "
Top of Rucu-Pichincha,	4,737	15,540	T.

Place.	Altitude, Meters.	Feet.	Method.
Cundurguachana,	4,090	13,254	1 B.
Top of Guagua-Pichincha,	4,787	15,704	T.
Bottom of the crater,	4,016	13,175	1 B.
Top of the mound in the crater,	4,087	13,408	1 "
Top of Panecillo,	3,050	10,006	2 "
Machángara, hacienda de las Monjas,	2,648	8,687	1 "
Bridge of Guápulo,	2,545	8,349	1 "
Church "	2,690	8,825	2 "
Tumbaco, plaza,	2,390	7,841	3 "
Puembo, "	2,484	8,149	2 "
Hacienda of Guachalá,	2,801	9,189	4 "
Cayambe, pueblo,	2,852	9,357	2 "
Top of Atacazo, edge of crater,	4,539	14,891	2 "
Arenal within crater,	4,242	13,916	2 "
Tambillo, tambo,	2,802	9,193	7 "
Aloac, pueblo,	2,922	9,586	2 "
Top of Corazon,	4,787	15,704	2 "
Machachi, tambo,	2,935	9,629	1 "
Gallocantana peak on Rumiñagui,	3,839	12,594	2 "
Bottom of the Caldera,	3,755	12,319	1 "
Peak between Capacocha and Sachacocha,	4,192	13,752	1 "
Hacienda of Pedregal,	3,531	11,584	1 "
Top of Pasochoa,	4,255	13,959	2 "
" Ilaló,	3,161	10,370	2 "
Alangasí, plaza,	2,587	8,487	1 "
Hacienda of Sñr. Jijon, Chillo,	2,518	8,261	1 "

ART. XXXVI.—*Inaugural Address before the British Association at Edinburgh, August 2d ;* by Sir WILLIAM THOMPSON, President of the Association.

Kew Observatory.

* * * One of the most valuable services to science which the British Association has performed has been the establishment, and the twenty-nine years maintenance, of its observatory. The Royal Meteorological Observatory of Kew was built originally for a sovereign of England who was a zealous amateur of astronomy. George the Third used continually to repair to it when any celestial phenomenon of peculiar interest was to be seen: and a manuscript book still exists filled with observations written into it by his own hand. After the building had been many years unused, it was granted, in the year 1842, by the Commissioners of her Majesty's Woods and Forests, on application of Sir Edward Sabine, for the purpose of continuing observations (from which he had already deduced important results) regarding the vibration of a pendulum

in various gases, and for the purpose of promoting pendulum observations in all parts of the world. The Government granted only the building—no funds for carrying on the work to be done in it. The Royal Society was unable to undertake the maintenance of such an observatory; but, happily for science, the zeal of individual fellows of the Royal Society and members of the British Association gave the initial impulse, supplied the necessary initial funds, and recommended their new institution successfully to the fostering care of the British Association. The work of the Kew Observatory has, from the commencement, been conducted under the direction of a committee of the British Association: and annual grants from the funds of the Association have been made toward defraying its expenses up to the present time. To the initial object of pendulum research was added continuous observations of the phenomena of meteorology and terrestrial magnetism, and the construction and verification of thermometers, barometers, and magnetometers designed for accurate measurement. The magnificent services which it has rendered to science are so well known that any statement of them which I could attempt on the present occasion would be superfluous. Their value is due in a great measure to the indefatigable zeal and the great ability of two Scotchmen, both from Edinburgh, who successfully held the office of Superintendent of the Obsertory of the British Association—Mr. Welsh for nine years, until his death in 1859, and Dr. Balfour Stewart from then until the present time. Fruits of their labors are to be found all through our volumes of Reports for these twenty-one years.

The institution now enters on a new stage of its existence. The noble liberality of a private benefactor, one who has labored for its welfare with self-sacrificing devotion unintermittingly from within a few years of its creation, has given it a permanent independence, under the general management of a committee of the Royal Society. Mr. Gassiot's gift of 10,000*l.* secures the continuance at Kew of the regular operation of the self-recording instruments for observing the phenomena of terrestrial magnetism and meteorology, without the necessity for further support from the British Association.

Physical Observatories and Laboratories.

The success of the Kew Magnetic and Meteorological Observatory affords an example of the great gain to be earned for science by the foundation of physical observatories and laboratories for experimental research, to be conducted by qualified persons, whose duties should be, not teaching, but experimenting. Whether we look to the honor of England, as a nation which ought always to be the foremost in promoting physical science, or to those vast economical advantages which must accrue from such establishments, we cannot but feel that experimental re-

search ought to be made with us an object of national concern, and not left, as hitherto, exclusively to the private enterprise of self-sacrificing amateurs, and the necessarily inconsecutive action of our present governmental departments and of casual committees. The Council of the Royal Society of Edinburgh has moved for this object in a memorial presented by them to the Royal Commision on Scientific Education and the Advancement of Science. The Continent of Europe is referred to for an example, to be followed with advantage in this country, in the following words:—

"On the continent there exist certain institutions, fitted with instruments, apparatus, chemicals, and other appliances, which are meant to be, and which are made, available to men of science, to enable them, at a moderate cost, to pursue original researches."

This statement is fully corroborated by information, on good authority, which I have received from Germany, to the effect that in Prussia "every university, every polytechnical academy, every industrial school (*Realschule* and *Gewerbeschule*), most of the grammer-schools, in a word, nearly all the schools superior in rank to the elementary schools of the common people, are supplied with chemical laboratories and a collection of philosophical instruments and apparatus, access to which is most liberally granted by the directors of those schools, or the teachers of the respective disciplines, to any person qualified, for *scientific experiments*. In consequence, though there exist no particular institutions like those mentioned in the memorial, there will scarcely be found a town exceeding 5,000 inhabitants but offers the possibility of *scientific explorations* at no other cost than reimbursement of the expense for the materials wasted in the experiments."

Further, with reference to a remark in the memorial to the effect that in respect to the promotion of science, the British Government confines its action almost exclusively to scientific instruction, and fatally neglects the advancement of science, my informant tells me that, in Germany, "professors, preceptors, and teachers of secondary schools are engaged on account of their skilfulness in *teaching*; but professors of universities are never engaged unless they have already proved, *by their own investigations*, that they are to be relied upon for the *advancement of science*. Therefore every shilling spent for instruction in universities is at the same time profitable to the advancement of science."

The physical laboratories which have grown up in the Universities of Glasgow and Edinburgh, and in Owen's College, Manchester, show the want felt of colleges of research; but they go but infinitesimally toward supplying it, being absolutely destitute of means, material or personal, for advancing science

except at the expense of volunteers, or securing that volunteers shall be found to continue even such little work as at present is carried on.

The whole of Andrew's splendid work in Queen's College, Belfast, has been done under great difficulties and disadvantages, and at great personal sacrifices; and up to the present time there is not a student's physical laboratory in any one of the Queen's Colleges in Ireland—a want which surely ought not to remain unsupplied. Each of these institutions (the four Scotch universities, the three Queen's Colleges, and Owen's College, Manchester) requires two professors of Natural Philosophy—one who shall be responsible for the teaching, the other for the advancement of science by experiment. The University of Oxford has already established a physical laboratory. The munificence of its Chancellor is about to supply the University of Cambridge with a splendid laboratory, to be constructed under the eye of Prof. Clerk Maxwell. On this subject I shall say no more at present, but simply read a sentence which was spoken by Lord Milton in the first Presidential Address to the British Association, when it met at York in the year 1831:—"In addition to other more direct benefits, these meetings [of the British Association], I hope, will be the means of impressing on the Government the conviction, that the love of scientific pursuits, and the means of pursuing them, are not confined to the metropolis; and I hope that when the Government is fully impressed with the knowledge of the great desire entertained to promote science in every part of the empire, they will see the necessity of affording it due encouragement, and of giving every proper stimulus to its advancement."

Besides abstracts of papers read, and discussions held, before the Sections, the annual Reports of the British Association contain a large mass of valuable matter of another class. It was an early practice of the Association, a practice that might well be further developed, to call occasionally for a special report on some particular branch of science from a man eminently qualified for the task. The reports received in compliance with these invitations have all done good service in their time, and they remain permanently useful as landmarks in the history of science. Some of them have led to vast practical results; others of a more abstract character are valuable to this day as powerful and instructive condensations and expositions of the branches of science to which they relate. I cannot better illustrate the two kinds of efficiency realized in this department of the Association's work than by referring to Cayley's 'Report on Abstract Dynamics,' and Sabine's 'Report on Terrestrial Magnetism,' (1838).

To the great value of the former, personal experience of benefit received enables me, and gratitude impels me, to testify. In a few pages full of precious matter, the generalized dynamical equations of Lagrange, the great principle evolved from Maupertius' "least action" by Hamilton, and the later developments and applications of the Hamiltonian principle by other authors, are described by Cayley so suggestively that the reading of thousands of quarto pages of papers scattered through the *transactions* of the various learned societies of Europe is rendered superfluous for any one who desires only the essence of these investigations, with no more of detail than is necessary for a thorough and practical understanding of the subject.

Terrestrial Magnetism.

Sabine's Report of 1838 concludes with the following sentence:—"Viewed in itself and its various relations, the magnetism of the earth cannot be counted less than one of the most important branches of the physical history of the planet we inhabit; and we may feel quite assured that the completion of our knowledge of its distribution on the surface of the earth would be regarded by our contemporaries and by posterity as a fitting enterprise of a maritime people, and a worthy achievement of a nation which has ever sought to rank foremost in every arduous and honorable undertaking." An immediate result of this report was that the enterprise which it proposed was recommended to the Government by a joint Committee of the British Association and the Royal Society with such success, that Capt. James Ross was sent in command of the Erebus and Terror to make a magnetic survey of the Antarctic regions, and to plant on his way three magnetical and meteorological observatories, at St. Helena, the Cape, and Van Diemen's Land. A vast mass of precious observations, made chiefly on board ship, were brought home from this expedition. To deduce the desired results from them, it was necessary to eliminate the disturbance produced by the ship's magnetism; and Sabine asked his friend Archibald Smith to work out from Poisson's mathematical theory, then the only available guide, the formulæ required for the purpose. This voluntary task Smith executed skillfully and successfully. It was the beginning of a series of labors carried on with most remarkable practical tact, with thorough analytical skill, and with a rare extreme of disinterestedness, in the intervals of an arduous profession, for the purpose of perfecting and simplifying the correction of the mariner's compass —a problem which had become one of vital importance for navigation, on account of the introduction of iron ships. Edition after edition of the 'Admiralty Compass Manual' has been produced by the able Superintendent of the Compass Department, Capt. Evans, containing chapters of mathematical investi-

gation and formulæ by Smith, on which depend wholly the practical analysis of compass observations and rules for the safe use of the compass in navigation. I firmly believe that it is to the thoroughly scientific method thus adopted by the Admiralty that no iron ship of her Majesty's Navy has ever been lost through errors of the compass. The 'British Admiralty Compass Manual' is adopted as a guide by all the navies of the world. It has been translated into Russian, German, and Portuguese; and it is at present being translated into French. The British Association may be gratified to know that the possibility of navigating ironclad war-ships with safety depends on the application of scientific principles given to the world by three mathematicians, Poisson, Airy, and Archibald Smith.

Returning to the science of terrestrial magnetism, we find in the reports of early years of the British Association ample evidence of its diligent cultivation. Many of the chief scientific men of the day from England, Scotland, and Ireland found a strong attraction to the Association in the facilities which it afforded to them for coöperating in their work on this subject. Lloyd, Phillips, Fox, Ross, and Sabine made magnetic observations all over Great Britain; and their results, collected by Sabine, gave for the first time an accurate and complete survey of terrestrial magnetism over the area of this island. I am informed, by Prof. Phillips, that, in the beginning of the Association, Herschel, though a "sincere well-wisher," felt doubt as to the general utility and probable success of the plan and purpose proposed; but his zeal for terrestrial magnetism brought him from being merely a sincere well-wisher to join actively and cordially in the work of the Association. "In 1838 he began to give effectual aid in the great question of magnetical observatories, and was indeed foremost among the supporters of that which is really Sabine's great work. At intervals, until about 1858, Herschel continued to give effectual aid." Sabine has carried on his great work without intermission to the present day; thirty years ago he gave to Gauss a large part of the data required for working out the spherical harmonic analysis of terrestrial magnetism over the whole earth. A recalculation of the harmonic analysis for the altered state of terrestrial magnetism of the present time has been undertaken by Adams. He writes to me that he has "already begun some of the introductory work, so as to be ready, when Sir Edward Sabine's tables of the values of the magnetic elements deduced from observation are completed, at once to make use of them," and that he intends to take into account terms of at least one order beyond those included by Gauss. The form in which the requisite data are to be presented to him is a magnetic chart of the whole surface of the globe. Materials from scientific trav-

elers of all nations, from our home magnetic observatories, from the magnetic observatories of St. Helena, the Cape, Van Diemen's Land, and Toronto, and from the scientific observatories of other countries, have been brought together by Sabine. Silently, day after day, night after night, for a quarter of a century, he has toiled with one constant assistant always by his side, to reduce these observations and prepare for the great work. At this moment, while we are here assembled, I believe that, in their quiet summer retirement in Wales, Sir Edward and Lady Sabine are at work on the magnetic chart of the world. If two years of life and health are granted to them, science will be provided with a key which must powerfully conduce to the ultimate opening up of one of the most refractory enigmas of cosmical physics, the cause of terrestrial magnetism.

To give any sketch, however slight, of scientific investigation performed during the past year, would, even if I were competent for the task, far exceed the limits within which I am confined on the present occasion. A detailed account of work done and knowledge gained in science Britain ought to have every year. The *Journal of the Chemical Society* and the *Zoological Record* do excellent service by giving abstracts of all papers published in their departments. The admirable example afforded by the German *Fortschritte* and *Jahresbericht* is before us; but hitherto, so far as I know, no attempt has been made to follow it in Britain. It is true that several of the annual volumes of the *Jahresbericht* were translated; but a translation, published necessarily at a considerable interval of time after the original, cannot supply the want. An independent British publication is for many obvious reasons desirable. The two publications, in German and English, would, both by their differences and by their agreements, illustrate the progress of science more correctly and usefully than any single work could do, even if appearing simultaneously in the two languages. It seems to me that to promote the establishment of a British year-book of science is an object to which the powerful action of the British Association would be thoroughly appropriate.

In referring to recent advances in several branches of science, I simply choose some of these which have struck me as most notable.

Accurate and minute measurement.

Accurate and minute measurement seems to the non-scientific imagination a less lofty and dignified work than looking for something new. But nearly all the grandest discoveries of science have been but the rewards of accurate measurement and patient long-continued labor in the minute sifting of numerical results. The popular idea of Newton's grandest discovery is that the theory of gravitation flashed into his mind, and so the

discovery was made. It was by a long train of mathematical calculation, founded on results accumulated through prodigious toil of practical astronomers, that Newton first demonstrated the forces urging the planets towards the Sun, determined the magnitudes of those forces, and discovered that a force following the same law of variation with distance urges the Moon toward the Earth. *Then* first, we may suppose, came to him the idea of the universality of gravitation; but when he attempted to compare the magnitude of the force on the Moon with the magnitude of the force of gravitation of a heavy body of equal mass at the Earth's surface, he did not find the agreement which the law he was discovering required. Not for years after would he publish his discovery as made. It is recounted that, being present at a meeting of the Royal Society, he heard a paper read, describing geodesic measurement by Picard, which led to a serious correction of the previously accepted estimate of the Earth's radius. This was what Newton required. He went home with the result, and commenced his calculations, but felt so much agitated that he handed over the arithmetical work to a friend: then (and not when, sitting in a garden, he saw an apple fall) did he ascertain that gravitation keeps the Moon in her orbit.

Faraday's discovery of specific inductive capacity, which inaugurated the new philosophy tending to discard action at a distance, was the result of minute and accurate measurement of electric forces.

Joule's discovery of thermo-dynamic law through the regions of electro-chemistry, electro-magnetism, and elasticity of gases, was based on a delicacy of thermometry which seemed simply impossible to some of the most distinguished chemists of the day.

Andrew's discovery of the continuity between the gaseous and liquid states was worked out by many years of laborious and minute measurement of phenomena scarcely sensible to the naked eye.

Great service has been done to science by the British Association in promoting accurate measurement in various subjects. The origin of exact science in terrestrial magnetism is traceable to Gauss's invention of methods of finding the magnetic intensity in absolute measure. I have spoken of the great work done by the British Association in carrying out the application of this invention in all parts of the world. Gauss's colleague in the German Magnetic Union, Weber, extended the practice of absolute measurement to electric currents, the resistance of an electric conductor, and the electromotive force of a galvanic element. He showed the relation beween electrostatic and electromagnetic units for absolute measurement, and made the

beautiful discovery that resistance, in absolute electromagnetic measure, and the reciprocal of resistance, or, as we call it, "conducting power," in electrostatic measure, are each of them a velocity. He made an elaborate and difficult series of experiments to measure the velocity which is equal to the conducting power in electrostatic measure, and at the same time to the resistance in electromagnetic measure, in one and the same conductor. Maxwell, in making the first advance along a road of which Faraday was the pioneer, discovered that this velocity is physically related to the velocity of light, and that, on a certain hypothesis regarding the elastic medium concerned, it may be exactly equal to the velocity of light. Weber's measurement verifies approximately this equality, and stands in science *monumentum ære perennius*, celebrated as having suggested this most grand theory, and as having afforded the first quantitative test of the recondite properties of matter on which the relations between electricity and light depend. A re-measurement of Weber's critical velocity on a new plan by Maxwell himself, and the important correction of the velocity of light by Foucault's laboratory experiments, verified by astronomical observation, seems to show a still closer agreement. The most accurate possible determination of Weber's critical velocity is just now a primary object of the Association's Committee on Electric Measurement; and it is at present premature to speculate as to the closeness of the agreement between that velocity and the velocity of light. This leads me to remark how much science, even in its most lofty speculations, gains in return for benefits conferred by its application to promote the social and material welfare of man. Those who perilled and lost their money in the original Atlantic telegraph were impelled and supported by a sense of the grandeur of their enterprise, and of the world-wide benefits which must flow from its success; they were, at the same time, not unmoved by the beauty of the scientific problem directly presented to them; but they little thought that it was to be immediately, through their work, that the scientific world was to be instructed in a long-neglected and discredited fundamental electric discovery of Faraday's, or that again, when the assistance of the British Association was invoked to supply their electricians with methods for absolute measurement (which they found necessary to secure the best economical return for their expenditure, and to obviate and detect those faults in their electric material which had led to disaster), they were laying the foundation for accurate electric measurement in every scientific laboratory in the world, and initiating a train of investigation which now sends up branches into the loftiest regions and subtlest ether of natural philosophy. Long may the British Association continue a bond of union, and a

medium for the interchange of good offices between science and the world!

Kinetic Theory of Gases—Atoms.

The greatest achievement yet made in molecular theory of the properties of matter is the kinetic theory of gases, shadowed forth by Lucretius, definitely stated by Daniel Bernoulli, largely developed by Herapath, made a reality by Joule, and worked out to its present advanced state by Clausius and Maxwell. Joule, from his dynamical equivalent of heat, and his experiments upon the heat produced by the condensation of gas, was able to estimate the average velocity of the ultimate molecules or atoms composing it. His estimate for hydrogen was 6,225 feet per second at temperature 60° Fahr., and 6,055 feet per second at the freezing point. Clausius took fully into account the impacts of molecules on one another, and the kinetic energy of *relative* motions of the matter constituting an individual atom. He investigated the relation between their diameters, the number in a given space, and the mean length of path from impact to impact, and so gave the foundation for estimates of the absolute dimensions of atoms, to which I shall refer later. He explained the slowness of gaseous diffusion by the mutual impacts of the atoms, and laid a secure foundation for a complete theory of the diffusion of fluids, previously a most refractory enigma. The deeply penetrating genius of Maxwell brought in viscosity and thermal conductivity, and thus completed the dynamical explanation of all the known properties of gases, except their electric resistance and brittleness to electric force.

No such comprehensive molecular theory had ever been even imagined before the nineteenth century. Definite and complete in its area as it is, it is but a well-drawn part of a great chart, in which all physical science will be represented with every property of matter shown in dynamical relation to the whole. The prospect we now have of an early completion of this chart, is based on the assumption of atoms. But there can be no permanent satisfaction to the mind in explaining heat, light, elasticity, diffusion, electricity and magnetism in gases, liquids, and solids, and describing precisely the relations of these different states of matter to one another by statistics of great numbers of atoms, when the properties of the atom itself are simply assumed. When the theory, of which we have the first installment in Clausius and Maxwell's work, is complete, we are but brought face to face with a superlatively grand question,—what is the inner mechanism of the atom?

In the answer to this question we must find the explanation not only of the atomic elasticity, by which the atom is a chronometric vibrator, according to Stoke's discovery, but of chemical affinity and of the differences of quality of different chemical

elements, at present a mere mystery in science. Helmholtz's exquisite theory of vortex-motion in an incompressible frictionless liquid has been suggested as a finger-post, pointing a way which may possibly lead to a full understanding of the properties of atoms, carrying out the grand conception of Lucretius, who "admits no subtle ethers, no variety of elements with fiery, or watery, or light, or heavy principles; nor supposes light to be one thing, fire another, electricity a fluid, magnetism a vital principle; but treats all phenomena as mere properties or accidents of simple matter." This statement I take from an admirable paper on the atomic theory of Lucretius, which appeared in the *North British Review* for March, 1868, containing a most interesting and instructive summary of ancient and modern doctrine regarding atoms. Allow me to read from that article one other short passage, finely describing the present aspect of atomic theory:—"The existence of the chemical atom, already quite a complex little world, seems very probable; and the description of the Lucretian atom is wonderfully applicable to it. We are not wholly without hope that the real weight of each such atom may some day be known—not merely the relative weight of the several atoms, but the number in a given volume of any material; that the form and motion of the parts of each atom and the distances by which they are separated may be calculated; that the motions by which they produce heat, electricity, and light may be illustrated by exact geometrical diagrams; and that the fundamental properties of the intermediate and possibly constituent medium may be arrived at. Then the motion of planets and music of the spheres will be neglected for a while in admiration of the maze in which the tiny atoms run."

Even before this was written some of the anticipated results had been partially attained. Loschmidt in Vienna had shown, and not much later Stoney independently in England showed, how to deduce from Clausius and Maxwell's kinetic theory of gases a superior limit to the number of atoms in a given measurable space. I was unfortunately quite unaware of what Loschmidt and Stoney had done when I made a similar estimate on the same foundation, and communicated it to *Nature*, in an article 'On the size of atoms.' But questions of personal priority, however interesting they may be to the persons concerned, sink into insignificance in the prospect of any gain of deeper insight into the secrets of nature. The triple coincidence of independent reasoning in this case is valuable as confirmation of a conclusion violently contravening ideas and opinions which had been almost universally held regarding the dimensions of the molecular structure of matter. Chemists and other naturalists had been in the habit of evading questions as to the

hardness or indivisibility of atoms by virtually assuming them to be infinitely small and infinitely numerous. We must now no longer look upon the atom, with Boscovich, as a mystic point endowed with inertia and the attribute of attracting or repelling other such centers with forces depending upon the intervening distances (a supposition only tolerated with the tacit assumption that the inertia and attraction of each atom is infinitely small and the number of atoms infinitely great), nor can we agree with those who have attributed to the atom occupation of space with infinite hardness and strength (incredible in any finite body); but we must realize it as a piece of matter of measurable dimensions, with shape, motion, and laws of action, intelligible subjects of scientific investigation.

Spectrum Analysis.

The prismatic analysis of light discovered by Newton was estimated by himself as being "the oddest, if not the most considerable, detection which had hitherto been made in the operations of nature."

Had he not been deflected from the subject, he could not have failed to obtain a pure spectrum; but this, with the inevitably consequent discovery of the dark lines, was reserved for the nineteenth century. Our fundamental knowledge of the dark lines is due solely to Fraunhofer. Wollaston saw them, but did not discover them. Brewster labored long and well to perfect the prismatic analysis of sunlight; and his observations on the dark bands produced by the absorption of interposed gases and vapors laid important foundations for the grand superstructure which he scarcely lived to see. Piazzi Smyth, by spectroscopic observation performed on the Peak of Teneriffe, added greatly to our knowledge of the dark lines produced in the solar spectrum by the absorption of our own atmosphere. The prism became an instrument for chemical qualitative analysis in the hands of Fox, Talbot and Herschel, who first showed how through it the old "blowpipe test," or generally the estimation of substances from the colors which they give to flames, can be prosecuted with an accuracy and a discriminating power not to be attained when the color is judged by the unaided eye. But the application of this test to solar and stellar chemistry had never, I believe, been suggested, either directly or indirectly, by any other naturalist, when Stokes taught it to me in Cambridge, at some time prior to the summer of 1852. The observational and experimental foundations on which he built were:—

(1) The discovery by Fraunhofer of a coincidence between his double dark line D of the solar spectrum and a double bright line which he observed in the spectra of ordinary artificial flames.

(2) A very rigorous experimental test of this coincidence by Prof. W. H. Miller, which showed it to be accurate to an astonishing degree of minuteness.

(3) The fact that the yellow light given out when salt is thrown on burning spirit consists almost solely of the two nearly identical qualities which constitute that double bright line

(4) Observations made by Stokes himself, which showed the bright line D to be absent in a candle-flame when the wick was snuffed clean, so as not to project into the luminous envelope, and from an alcohol flame when the spirit was burned in a watch-glass. And,

(5) Foucault's admirable discovery (*L'Institut*, Feb. 7, 1849), that the voltaic arc between charcoal points is "a medium which emits the rays D on its own account, and at the same time absorbs them when they come from another quarter."

The conclusions, theoretical and practical, which Stokes taught me, and which I gave regularly afterwards in my public lectures in the University of Glasgow, were:—

(1) That the double line D, whether bright or dark, is due to vapor of sodium.

(2) That the ultimate atom of sodium is susceptible of regular elastic vibrations, like those of a tuning-fork or of stringed musical instruments; that like an instrument with two strings tuned to approximate unison, or an approximately circular elastic disc, it has two fundamental notes or vibrations of approximately equal pitch; and that the periods of these vibrations are precisely the periods of the two slightly different yellow lights constituting the double bright line D.

(3) That when vapor of sodium is at a high enough temperature to become itself a source of light, each atom executes these two fundamental vibrations simultaneously; and that therefore the light proceeding from it is of the two qualities constituting the double bright line D.

(4) That when vapor of sodium is present in space across which light from another source is propagated, its atoms, according to a well-known general principle of dynamics, are set to vibrate in either or both of those fundamental modes, if some of the incident light is of one or other of their periods, or some of one and some of the other; so that the energy of the waves of those particular qualities of light is converted into thermal vibrations of the medium, and dispersed in all directions, while light of all other qualities, even though very nearly agreeing with them, is transmitted with comparatively no loss.

(5) That Fraunhofer's double dark line D of solar and stellar spectra is due to the presence of vapor of sodium in atmospheres

surrounding the sun and those stars in whose spectra it had been observed.

(6) That other vapors than sodium are to be found in the atmospheres of sun and stars by searching for substances producing in the spectra of artificial flames bright lines coinciding with other dark lines of the solar and stellar spectra than the Fraunhofer line D.

The last of these propositions I felt to be confirmed (it was, perhaps, partly suggested) by a striking and beautiful experiment, admirably adapted for lecture illustrations, due to Foucault, which had been shown to me by M. Duboscque Soleil and the Abbé Moigno, in Paris, in the month of October, 1850. A prism and lenses were arranged to throw upon a screen an approximately pure spectrum of a vertical electric arc between charcoal poles of a powerful battery, the lower one of which was hollowed like a cup. When pieces of copper and pieces of zinc were separately thrown into the cup, the spectrum exhibited, in perfectly definite positions, magnificent well-marked bands of different colors characteristic of the two metals. When a piece of brass, compounded of copper and zinc, was put into the cup, the spectrum showed all the bands, each precisely in the place in which it had been seen when one metal or the other had been used separately.

It is much to be regretted that this great generalization was not published to the world twenty years ago. I say this, not because it is to be regretted that Ångström should have the credit of having, in 1853, published independently the statement that "an incandescent gas emits luminous rays of the same refrangibility as those which it can absorb"; or that Balfour Stewart should have been unassisted by it when, coming to the subject from a very different point of view, he made, in his extension of the 'Theory of Exchanges' (*Edin. Transactions*, 1858–59) the still wider generalization that the radiating power of every kind of substance is equal to its absorbing power for every kind of ray; or that Kirchhoff also should have, in 1859, independently discovered the same proposition, and shown its application to solar and stellar chemistry; but because we might now be in possession of the inconceivable riches of astronomical results which we expect from the next ten years' investigation by spectrum analysis, had Stokes given his theory to the world when it first occurred to him.

To Kirchhoff belongs, I believe, solely the great credit of having first actually sought for and found other metals than sodium in the sun by the method of spectrum analysis. His publication of October, 1859, inaugurated the practice of solar and stellar chemistry, and gave spectrum analysis an impulse to which in a great measure is due its splendidly successful cul-

tivation by the labors of many able investigators within the last ten years.

To prodigious and wearing toil of Kirchhoff himself, and of Angström, we owe large-scale maps of the solar spectrum, incomparably superior in minuteness and accuracy of delineation to anything ever attempted previously. These maps now constitute the standards of reference for all workers in the field. Plücker and Hittorf opened ground in advancing the physics of spectrum analysis, and made the important discovery of changes in the spectra of ignited gases produced by changes in the physical condition of the gas. The scientific value of the meetings of the British Association is well illustrated by the fact that it was through conversation with Plücker at the Newcastle meeting that Lockyer was first led into the investigation of the effects of varied pressure on the quality of the light emitted by glowing gas which he and Frankland have prosecuted with such admirable success. Scientific wealth tends to accumulation according to the law of compound interest. Every addition to knowledge of properties of matter supplies the naturalist with new instrumental means for discovering and interpreting phenomena of nature, which in their turn afford foundations for fresh generalizations, bringing gains of permanent value into the great storehouse of philosophy. Thus Frankland, led from observing the want of brightness of a candle burning in a tent on the summit of Mont Blanc, to scrutinize Davy's theory of flame, discovered that brightness without incandescent solid particles is given to a purely gaseous flame by augmented pressure, and that a dense ignited gas gives a spectrum comparable with that of the light from an incandescent solid or liquid. Lockyer joined him; and the two found that every incandescent substance gives a continuous spectrum—that an incandescent gas under varied pressure gives bright bars across the continuous spectrum, some of which, from the sharp, hard and fast lines observed where the gas is in a state of extreme attenuation, broaden out on each side into nebulous bands as the density is increased, and are ultimately lost in the continuous spectrum when the condensation is pushed on till the gas becomes a fluid no longer to be called gaseous. More recently they have examined the influence of temperature, and have obtained results which seem to show that a highly attenuated gas, which at a high temperature gives several bright lines, gives a smaller and smaller number of lines, of sufficient brightness to be visible, when the temperature is lowered, the density being kept unchanged. I cannot refrain here from remarking how admirably this beautiful investigation harmonizes with Andrews's great discovery of continuity between the gaseous and liquid states. Such things make the life-blood of science.

In contemplating them we feel as if led out from narrow waters of scholastic dogma to a refreshing excursion on the broad and deep ocean of truth, where we learn from the wonders we see that there are endlessly more and more glorious wonders still unseen.

Stokes's dynamical theory supplies the key to the philosophy of Frankland and Lockyer's discovery. Any atom of gas, when struck and left to itself, vibrates with perfect purity its fundamental note or notes. In a highly attenuated gas each atom is very rarely in collision with other atoms, and therefore is nearly at all times in a state of true vibration. Hence the spectrum of a highly attenuated gas consists of one or more perfectly sharp, bright lines, with a scarcely perceptible continuous gradation of prismatic color. In denser gas each atom is frequently in collision, but still is for much more time free, in intervals between collisions, than engaged in collision; so that not only is the atom itself thrown sensibly out of tune during a sensible proportion of its whole time, but the confused jangle of vibrations in every variety of period during the actual collision becomes more considerable in its influence. Hence bright lines in the spectrum broaden out somewhat, and the continuous spectrum becomes less faint. In still denser gas each atom may be almost as much time in collision as free, and the spectrum then consists of broad nebulous bands crossing a continuous spectrum of considerable brightness. When the medium is so dense that each atom is always in collision, that is to say, never free from influence of its neighbours, the spectrum will generally be continuous, and may present little or no appearance of bands, or even of maxima of brightness. In this condition the fluid can be no longer regarded as a gas, and we must judge of its relation to the vaporous or liquid states according to the critical conditions discovered by Andrews.

Spectroscopic Research in Astronomy.

While these great investigations of properties of matter were going on, naturalists were not idle with the newly-recognized power of the spectroscope at their service. Chemists soon followed the example of Bunsen in discovering new metals in terrestrial matter by the old blow-pipe and prism test of Fox, Talbot and Herschel. Biologists applied spectrum analysis to animal and vegetable chemistry, and to sanitary investigations. But it is in astronomy that spectroscopic research has been carried on with the greatest activity, and been most richly rewarded with results. The chemist and the astronomer have joined their forces. An astronomical observatory has now appended to it a stock of re-agents such as hitherto was only to be found in the chemical laboratory. A devoted corps of volunteers of all nations, whose motto might well be *Ubique*,

have directed their artillery to every region of the universe. The Sun, the spots on his surface, the corona and the red and yellow prominences seen round him during total eclipses, the moon, the planets, comets, auroras, nebulæ, white stars, yellow stars, red stars, variable and temporary stars, each, tested by the prism, was compelled to show its distinguishing prismatic colours. Rarely before in the history of science has enthusiastic perseverance directed by penetrative genius produced within ten years so brilliant a succession of discoveries. It is not merely the *chemistry* of sun and stars, as first suggested, that is subjected to analysis by the spectroscope. Their whole laws of being are now subjects of direct investigation; and already we have glimpses of their evolutional history through the stupendous power of this most subtle and delicate test. We had only solar and stellar chemistry; we now have solar and stellar physiology.

It is an old idea that the color of a star may be influenced by its motion relatively to the eye of the spectator, so as to be tinged with red if it moves from the earth, or blue if it moves toward the earth. William Allen Miller, Huggins, and Maxwell showed how, by aid of the spectroscope, this idea may be made the foundation of a method of measuring the relative velocity with which a star approaches to or recedes from the earth. The principle is, first to identify, if possible, one or more of the lines in the spectrum of the star, with a line or lines in the spectrum of sodium, or some other terrestrial substance, and then (by observing the star and the artificial light simultaneously by the same spectroscope) to find the difference, if any, between their refrangibilities. From this difference of refrangibility the ratio of the periods of the two lights is calculated, according to data determined by Fraunhofer from comparisons between the positions of the dark lines in the prismatic spectrum and in his own "interference spectrum" (produced by substituting for the prism a fine grating). A first comparatively rough application of the test by Miller and Huggins to a large number of the principal stars of our skies, including Aldebaran, α Orionis, β Pegasi, Sirius, α Lyræ, Capella, Arcturus, Pollux, Castor (which they had observed rather for the chemical purpose than for this), proved that not one of them had so great a velocity as 315 kilomètres per second to or from the earth, which is a *most momentous result in respect to cosmical dynamics.* Afterward Huggins made special observations of the velocity test, and succeeded in making the measurement in one case, that of Sirius, which he then found to be receding from the Earth at a rate of 66 kilomètres per second. This, corrected for the velocity of the Earth at the time of the observation, gave a velocity of Sirius, relatively to the Sun, amount-

ing to 47 kilomètres per second. The minuteness of the difference to be measured, and the smallness of the amount of light, even when the brightest star is observed, render the observation extremely difficult. Still, with such great skill as Mr. Huggins has brought to bear on the investigation, it can scarcely be doubted that velocities of many other stars may be measured. What is now wanted is, certainly not greater skill, perhaps not even more powerful instruments, but *more instruments and more observers.* Lockyer's applications of the velocity test to the relative motions of different gases in the Sun's photosphere, spots, chromosphere, and chromospheric prominences, and his observations of the varying spectra presented by the same substance as it moves from one position to another in the Sun's atmosphere, and his interpretations of these observations, according to the laboratory results of Frankland and himself, go far toward confirming the conviction that in a few years all the marvels of the Sun will be dynamically explained according to known properties of matter.

During six or eight precious minutes of time, spectroscopes have been applied to the solar atmosphere and to the corona seen round the dark disc of the Moon eclipsing the Sun. Some of the wonderful results of such observations, made in India on the occasion of the eclipse of August, 1868, were described by Prof. Stokes in a previous address. Valuable results have, through the liberal assistance given by the British and American Governments, been obtained also from the total eclipse of last December, notwithstanding a generally unfavorable condition of weather. It seems to have been proved that at least some sensible part of the light of the "corona" is a terrestrial atmospheric halo or dispersive reflexion of the light of the glowing hydrogen and "helium" round the Sun. (Frankland and Lockyer find the yellow prominences to give a very decided bright line not far from D, but hitherto not identified with any terrestrial flame. It seems to indicate a new substance, which they propose to call Helium.) I believe I may say, on the present occasion, when preparation must again be made to utilize a total eclipse of the Sun, that the British Association confidently trusts to our Government exercising the same wise liberality as heretofore in the interests of science.

Solar Heat.

The old nebular hypothesis supposes the solar system and other similar systems through the universe which we see at a distance as stars, to have originated in the condensation of fiery nebulous matter. This hypothesis was invented before the discovery of the thermo-dynamics, or the nebulæ would not have been supposed to be fiery; and the idea seems never to have occurred to any of its inventors or early supporters that the

matter, the condensation of which they supposed to constitute the Sun and stars, could have been other than fiery in the beginning. Mayer first suggested that the heat of the Sun may be due to gravitation; but he supposed meteors falling in to keep always generating the heat which is radiated year by year from the Sun. Helmholtz, on the other hand, adopting the nebular hypothesis, showed in 1854 that it was not necessary to suppose the nebulous matter to have been originally fiery, but that mutual gravitation between its parts may have generated the heat to which the present high temperature of the Sun is due. Further, he made the important observations that the potential energy of gravitation in the Sun is even now far from exhausted; but that with further and further shrinking more and more heat is to be generated, and that thus we can conceive the Sun even now to possess a sufficient store of energy to produce heat and light, almost as at present, for several million years of time future. It ought, however, to be added that this condensation can only follow from cooling, and therefore that Helmholtz's gravitational explanation of future Sun-heat amounts really to showing that the Sun's thermal capacity is enormously greater, in virtue of the mutual gravitation between the parts of so enormous a mass, than the sum of the thermal capacities of separate and smaller bodies of the same material and same total mass. Reasons for adopting this theory, and the consequences which follow from it, are discussed in an article 'On the Age of the Sun's Heat,' published in *Macmillan's Magazine* for March, 1862.

For a few years Mayer's theory of solar heat had seemed to me probable; but I had been led to regard it as no longer tenable, because I had been in the first place driven, by consideration of the very approximate constancy of the Earth's period of revolution round the Sun for the last 2,000 years, to conclude that "the principal source, perhaps the sole appreciably effective source of the Sun-heat, is in bodies circulating round the Sun at present inside of the Earth's orbit;" and because Le Verrier's researches on the motion of the planet Mercury, though giving evidence of a sensible influence attributable to matter circulating as a great number of small planets within his orbit round the Sun, showed that the amount of matter that could possibly be assumed to circulate at any considerable distance from the Sun must be very small; and therefore, "if the meteoric influx taking place at present is enough to produce any appreciable portion of the heat radiated away, it must be supposed to be from matter circulating round the Sun, within very short distances of his surface. The density of this meteoric cloud would have to be supposed so great that comets could scarcely have escaped as comets actually have escaped, showing

no discoverable effects of resistance, after passing his surface within a distance equal to one-eighth of his radius. All things considered, there seems little probability in the hypothesis that solar radiation is compensated to any appreciable degree, by heat generated by meteors falling in, at present; and, as it can be shown that no chemical theory is tenable, it must be concluded as most probable that the Sun is at present merely an incandescent liquid mass cooling."

Thus on purely astronomical grounds was I long ago led to abandon as very improbable the hypothesis that the Sun's heat is supplied dynamically from year to year by the influx of meteors. But now spectrum analysis gives proof finally conclusive against it.

Each meteor circulating round the Sun must fall in along a very gradual spiral path, and before reaching the Sun must have been for a long time exposed to an enormous heating effect from his radiation when very near, and must thus have been driven into vapor before actually falling into the Sun. Thus, if Mayer's hypothesis is correct, friction between vortices of meteoric vapors and the Sun's atmosphere must be the immediate cause of solar heat; and the velocity with which these vapors circulate round equatorial parts of the Sun must amount to 435 kilomètres per second. The spectrum test of velocity applied by Lockyer showed but a twentieth part of this amount as the greatest observed relative velocity between different vapors in the Sun's atmosphere.

At the first Liverpool Meeting of the British Association (1854), in advancing a gravitational theory to account for all the heat, light, and motions of the universe, I urged that the immediately antecedent condition of the matter of which the Sun and planets were formed, not being fiery, could not have been gaseous; but that it probably was solid, and may have been like the meteoric stones which we still so frequently meet with through space. The discovery of Huggins, that the light of the nebulæ, so far as hitherto sensible to us, proceeds from incandescent hydrogen and nitrogen gases, and that the heads of comets also give us light of incandescent gas, seems at first sight literally to fulfill that part of the nebular hypothesis to which I had objected. But a solution, which seems to me in the highest degree probable, has been suggested by Tait. He supposes that it may be by ignited gaseous exhalations proceeding from the collision of meteoric stones that nebulæ and the heads of comets show themselves to us; and he suggested, at a former meeting of the Association, that experiments should be made for the purpose of applying spectrum analysis to the light which has been observed in gunnery trials, such as those at Shoeburyness, when iron strikes against iron at a great velocity,

but varied by substituting for the iron various solid materials, metallic or stony. Hitherto this suggestion has not been acted upon; but surely it is one the carrying out of which ought to be promoted by the British Association.

Nature of Comets.

Most important steps have been recently made toward the discovery of the nature of comets; establishing with nothing short of certainty the truth of a hypothesis which had long appeared to me probable,—that they consist of groups of meteoric stones; accounting satisfactorily for the light of the nucleus, and giving a simple and rational explanation of phenomena presented by the tails of comets which had been regarded by the greatest astronomers as almost preternaturally marvelous. The meteoric hypothesis to which I have referred remained a mere hypothesis (I do not know that it was ever even published), until, in 1866, Schiaparelli calculated from observations on the August meteors, an orbit for these bodies which he found to agree almost perfectly with the orbit of the great comet of 1862, as calculated by Oppolzer; and so discovered and demonstrated that a comet consists of a group of meteoric stones. Prof. Newton, of Yale College, United States, by examining ancient records, ascertained that in periods of about thirty-three years, since the year 902, there have been exceptionally brilliant displays of the November meteors. It had long been believed that these interesting visitants came from a train of small detached planets circulating round the Sun, all in nearly the same orbit, and constituting a belt analogous to Saturn's ring; and that the reason for the comparatively large number of meteors which we observe annually about the 14th of November is, that at that time the Earth's orbit cuts through the supposed meteoric belt. Prof. Newton concluded from his investigation that there is a denser part of the group of meteors which extends over a portion of the orbit so great as to occupy about one-tenth or one-fifteenth of the periodic time in passing any particular point, and gave a choice of five different periods for the revolution of this meteoric stream round the Sun, any one of which would satisfy his statistical result. He further concluded that the line of nodes, that is to say, the line in which the plane of the meteoric belt cuts the plane of the Earth's orbit, has a progressive sidereal motion of about $52''{\cdot}4$ per annum. Here, then, was a splendid problem for the physical astronomer; and, happily, one well qualified for the task took it up. Adams, by the application of a beautiful method invented by Gauss, found that of the five periods allowed by Newton just one permitted the motion of the line of nodes to be explained by the disturbing influence of Jupiter, Saturn, and other planets. The period chosen on these grounds is $33\frac{1}{4}$

years. The investigation showed further that the form of the orbit is a long ellipse, giving for the shortest distance from the Sun 145 million kilomèters, and for longest distance 2,895 million kilomèters. Adams also wŏrked out the longitude of the perihelion and the inclination of the orbit's plane to the plane of the ecliptic. The orbit which he thus found agreed so closely with that of Temple's Comet I, 1866, that he was able to identify the comet and the meteoric belt.* The same conclusion had been pointed out a few weeks earlier by Schiaparelli, from calculations by himself, on the data supplied by direct observations on the meteors, and independently by Peters, from calculations by Leverrier on the same foundation. It is, therefore, thoroughly established that Temple's Comet I, 1866, consists of an elliptic train of minute planets, of which a few thousands or millions fall to the earth annually about the 14th of November, when we cross their track. We have probably not yet passed through the very nucleus or densest part; but thirteen times, in Octobers and Novembers, from October 13, A. D. 902, to November 14, 1866, inclusive (this last time having been correctly predicted by Prof. Newton), we have passed through a part of the belt greatly denser than the average. The densest part of the train, when near enough to us, is visible as the head of the comet. This astounding result, taken along with Huggins's spectroscopic observations on the light of the heads and tails of comets, confirms most strikingly Tait's theory of comets, to which I have already referred; according to which the comet, a group of meteoric stones, is self-luminous in its nucleus, on account of collisions among its constitutents, while its "tail" is merely a portion of the less dense part of the train illuminated by sunlight, and visible or invisible to us

* Signor Schiaparelli, Director of the Observatory of Milan, who, in a letter dated 31st of December, 1866, pointed out that the elements of the orbit of the *August* meteors, calculated from the observed position of their radiant point on the supposition of the orbit being a very elongated ellipse, agreed very closely with those of the orbit of Comet II, 1862, calculated by Dr. Oppolzer. In the same letter Schiaparelli gives elements of the orbit of the November meteors, but these were not sufficiently accurate to enable him to identify the orbit with that of any known comet. On the 21st of January, 1867, M. Leverrier gave more accurate elements of the orbit of the November meteors, and in the *Astronomische Nachrichten* of January 9, Mr. C. F. W. Peters, of Altona, pointed out that these elements closely agreed with those of Temple's Comets (I, 1866), calculated by Dr. Oppolzer, and on February 2, Schiaparelli, having re-calculated the elements of the orbit of the meteors, himself noticed the same agreement. Adams arrived quite independently at the conclusion that the orbit of 33¼ years period is the one which *must* be chosen, out of the five indicated by Prof. Newton. His calculations were sufficiently advanced before the letters referred to appeared, to show that the other four orbits offered by Newton were inadmissible. But the calculations to be gone through to find the secular motion of the node in such an elongated orbit as that of the meteors, were necessarily very long, so that they were not completed till about March, 1867. They were communicated in that month to the Cambridge Philosophical Society, and in the month following to the Astronomical Society.

according to circumstances, not only of density, degree of illumination, and nearness, but also of tactic arrangement, as of a flock of birds or the edge of a cloud of tobacco smoke! What prodigious difficulties are to be explained, you may judge from two or three sentences which I shall read from Herschel's Astronomy, and from the fact that even Schiaparelli seems still to believe in the repulsion. "There is, beyond question, some profound secret and mystery of nature concerned in the phenomenon of their tails. Perhaps it is not too much to hope that future observation, borrowing every aid from rational speculation, grounded on the progress of physical science generally (especially those branches of it which relate to the ethereal or imponderable elements), may enable us ere long to penetrate this mystery, and to declare whether it is really *matter* in the ordinary acceptation of the term which is projected from their heads with such extraordinary velocity, and if not *impelled*, at least *directed* in its course, by reference to the Sun, as its point of avoidance." "In no respect is the question as to the materiality of the tail more forcibly pressed on us for consideration than in that of the enormous sweep which it makes round the Sun in *perihelio* in a manner of a straight and rigid rod, *in defiance of the law of gravitation*, nay, even *of the received* laws of motion." "The projection of this ray . . . . to so enormous a length, in a single day, conveys an impression of the intensity of the forces acting to produce such a velocity of material transfer through space, such as no other natural phenomenon is capable of exciting. It is clear that *if we have to deal here with matter, such as we conceive it*, viz., *possessing inertia—at all*, it must be under the dominion of forces incomparably more energetic than gravitation, and quite of a different nature."

Think now of the admirable simplicity with which Tait's beautiful "sea-bird analogy," as it has been called, can explain all these phenomena.

Origin of Life.

The essence of science, as is well illustrated by astronomy and cosmical physics, consists in inferring antecedent conditions, and anticipating future evolutions, from phenomena which have actually come under observation. In biology the difficulties of successfully acting up to this ideal are prodigious. The earnest naturalists of the present day are, however, not appalled or paralyzed by them, and are struggling boldly and laboriously to pass out of the mere "Natural History stage" of their study, and bring zoology within the range of Natural Philosophy. A very ancient speculation, still clung to by many naturalists (so much so that I have a choice of modern terms to quote in expressing it) supposes that, under meteorological conditions very different from the present, dead matter may have run together,

or crystallized or fermented into "germs of life," or "organic cells," or "protoplasm." But science brings a vast mass of inductive evidence against this hypothesis of spontaneous generation, as you have heard from my predecessor in the Presidential chair. Careful enough scrutiny has, in every case up to the present day, discovered life as antecedent to life. Dead matter cannot become living without coming under the influence of matter previously alive. This seems to me as sure a teaching of science as the law of gravitation. I utterly repudiate, as opposed to all philosophical uniformitarianism, the assumption of "different meteorolgical conditions"—that is to say, somewhat different vicissitudes of temperature, pressure, moisture, gaseous atmosphere—to produce or to permit that to take place by force or motion of dead matter alone, which is a direct contravention of what seems to us biological law. I am prepared for the answer, "our code of biological laws is an expression of our ignorance as well as of our knowledge." And I say, yes; search for spontaneous generation out of inorganic materials; let any one not satisfied with the purely negative testimony of which we have now so much against it, throw himself into the inquiry. Such investigations as those of Pasteur, Pouchet, and Bastian are among the most interesting and momentous in the whole range of Natural History, and their results, whether positive or negative, must richly reward the most careful and laborious experimenting. I confess to being deeply impressed by the evidence put before us by Prof. Huxley, and I am ready to adopt, as an article of scientific faith, true through all space and through all time, that life proceeds from life, and from nothing but life.

How, then, did life originate on the earth? Tracing the physical history of the earth backward, on strict dynamical principles, we are brought to a red-hot melted globe, on which no life could exist. Hence, when the earth was first fit for life, there was no living thing on it. There were rocks solid and disintegrated, water, air all round, warmed and illuminated by a brilliant sun, ready to become a garden. Did grass and trees and flowers spring into existence, in all the fullness of ripe beauty, by a fiat of Creative Power? or did vegetation, growing up from seed sown, spread and multiply over the whole earth? Science is bound, by the everlasting law of honor, to face fearlessly every problem which can fairly be presented to it. If a probable solution, consistent with the ordinary course of nature, can be found, we must not invoke an abnormal act of Creative Power. When a lava stream flows down the sides of Vesuvius or Etna it quickly cools and becomes solid; and after a few weeks or years it teems with vegetable and animal life, which for it originated by the transport of seeds and ova

and by the migration of individual living creatures. When a volcanic island springs up from the sea, and after a few years is found clothed with vegetation, we do not hesitate to assume that seeds has been wafted to it through the air, or floated to it on rafts. Is it not possible, and if possible, is it not probable, that the beginning of vegetable life on the earth is to be similarly explained? Every year thousands, probably millions, of fragments of solid matter fall upon the earth—whence come these fragments? What is the previous history of any one of them? Was it created in the beginning of time an amorphous mass? This idea is so unacceptable that, tacitly or explicitly, all men discard it. It is often assumed that all, and it is certain that some, meteoric stones are fragments which had been broken off from greater masses and launched free into space. It is as sure that collisions must occur between great masses moving through space as it is that ships, steered without intelligence directed to prevent collision, could not cross and re-cross the Atlantic for thousands of years with immunity from collisions. When two great masses come into collision in space, it is certain that a large part of each is melted; but it seems also quite certain that in many cases a large quantity of *débris* must be shot forth in all directions, much of which may have experienced no greater violence than individual pieces of rock experience in a land-slip or in blasting by gunpowder. Should the time when this earth comes into collision with another body, comparable in dimensions to itself, be when it is still clothed as at present with vegetation, many great and small fragments carrying seed and living plants and animals would undoubtedly be scattered through space. Hence and because we all confidently believe that there are at present, and have been from time immemorial, many worlds of life besides our own, we must regard it as probable in the highest degree that there are countless seed-bearing meteoric stones moving about through space. If at the present instant no life existed upon this earth, one such stone falling upon it might, by what we blindly call *natural* causes, lead to its becoming covered with vegetation. I am fully conscious of the many scientific objections which may be urged against this hypothesis, but I believe them to be all answerable. I have already taxed your patience too severely to allow me to think of discussing any of them on the present occasion. The hypothesis that life originated on this earth through moss-grown fragments from the ruins of another world may seem wild and visionary; all I maintain is that it is not unscientific.

From the earth stocked with such vegetation as it could receive meteorically, to the earth teeming with all the endless variety of plants and animals which now inhabit it, the step is

prodigious; yet, according to the doctrine of continuity, most ably laid before the Association by a predecessor in this chair (Mr. Grove), all creatures now living on earth have proceeded by orderly evolution from some such origin. Darwin concludes his great work on 'The Origin of Species' with the following words:—"It is interesting to contemplate an entangled bank clothed with many plants of many kinds, with birds singing on the bushes, with various insects flitting about, and with worms crawling through the damp earth, and to reflect that these elaborately constructed forms, so different from each other, and dependent on each other in so complex a manner, have all been produced by laws acting around us." . . . . "There is grandeur in this view of life with its several powers, having been originally breathed by the Creator into a few forms or into one; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning, endless forms, most beautiful and most wonderful, have been and are being evolved." With the feeling expressed in these two sentences I most cordially sympathize. I have omitted two sentences which come between them, describing briefly the hypothesis of "the origin of species by natural selection," because I have always felt that this hypothesis does not contain the true theory of evolution, if evolution there has been in biology. Sir John Herschel, in expressing a favorable judgment on the hypothesis of zoological evolution, with, however, some reservation in respect to the origin of man, objected to the doctrine of natural selection, that it was too like the Laputan method of making books, and that it did not sufficiently take into account a continually guiding and controlling intelligence. This seems to me a most valuable and instructive criticism. I feel profoundly convinced that the argument of design has been greatly too much lost sight of in recent zoological speculations. Reaction against the frivolities of teleology, such as are to be found, not rarely, in the notes of the learned commentators on Paley's 'Natural Theology,' has, I believe, had a temporary effect in turning attention from the solid and irrefragable argument so well put forward in that excellent old book. But overpoweringly strong proofs of intelligent and benevolent design lie all round us, and if ever perplexities, whether metaphysical or scientific, turn us away from them for a time, they come back upon us with irresistible force, showing to us through nature and the influence of a free will, and teaching us that all living beings depend on one ever acting Creator and Ruler.

ART. XXXVII.—*On some new Silurian Crinoids and Shells;* by F. B. MEEK.

DENDROCRINUS CASEI *Meek.*

Compare *Pentacrinite* Christy, 1848; Letters on Geology, Plate II.

Column very distinctly pentagonal, the angles at the connection with the body being continuous with strong ridges passing up the sutures between the basal pieces and to the middle of the subradials. Body pentagonal-obconic, a little wider above than high. Base wider than high, strongly pentagonal, being deeply excavated up the middle of each piece, and very prominent at the lateral sutures; basal pieces wider below than high, pentagonal in form, with the mesial angle above salient, and the superior lateral sloping sides much longer than the lateral. Subradial pieces of moderate size, those seen, hexagonal in form, and all very convex in the middle, from which point they send one strongly elevated ridge to meet others on each of the surrounding plates, and others coming up the sutures between the basals, while on each side of all of these ridges, excepting sometimes those passing to the first radials above, there is usually a smaller, less elevated ridge; the surface of the body being thus divided by these ridges into very profoundly excavated triangular spaces, in the middle of which the corners of the body plates meet. First radial pieces, excepting one on the anal side, larger than the subradials, about as wide as high, with a general pentagonal outline, the upper side being longest and deeply excavated for the reception of the comparatively narrow free radials or arm pieces; one on the right of the anal series, shorter than the others, pentagonal in form, and supporting above another larger radial that is included as a part of the wall of the body, and corresponds with the first radials in the other rays, excepting that it is shorter; all convex and sending a strong ridge to each of the contiguous body plates below, while a number of much smaller ridges pass horizontally across from one to another of these pieces on each side. Arms or free rays, comparatively rather narrow, distinctly rounded on the outer or dorsal side, and composed of transversely oblong pieces that are about twice as wide as long below the first bifurcation; in the first ray on the right of the anal series, bifurcating on the fourth free piece, beyond which they are seen to be long and composed of proportionally narrow pieces; but their mode of bifurcation, if they divide again, and their structure in the other rays, cannot be made out from the specimens at hand. Anal series unknown.

Ventral extension of the body more than four times as long as the latter, and as seen flattened by pressure, of greater breadth; as usual, composed of numerous small equal hexagonal, alternately interlocking pieces, that are strengthened by little oblique costæ so arranged as to present an ascending zigzag appearance. Surface, excepting the strong costæ of the body plates, and the smaller ones of the ventral part, without ornamentation.

Height of body to top of first radials, 0·39 inch, greatest breadth at top, 0·32 inch. Length of incomplete ventral extension, 1·95 inches; breadth of same as flattened, near upper end, 0·65 inch; breadth of arms below the first bifurcation, 0·12 inch.

This beautiful species seems to be a true *Dendrocrinus*, as it can be seen to have two of the primary radials on the right of the anal series, included as a part of the walls of the body, while all its other parts seem to conform to the structure of that group. The differences between *Dendrocrinus* and *Poteriocrinites* are not very great, and it is thought by some that the former should stand only as a subgenus under *Poteriocrinites*. If so, the name of this species when written in full, would be *Poteriocrinites* (*Dendocrinus*) *Casei*. In general appearance it resembles *Palæocrinus angulatus* of Billings, but it differs in having the costæ of its body, in part, with a smaller one on each side; while its column is very much more strongly pentagonal. Of course it also differs in the generic character of having its ventral part extended upward nearly or quite as long as the arms. *

It may be that the species here described is the same figured by Mr. Christy, in his "Letters on Geology," as a Pentacrinite (without a specific name), as it came from the same horizon, and from about the same region of country. It does not, however, agree *exactly* with his figure in details.

The specific name is given in honor of L. B. Case, Esq., of Richmond, Indiana, to whom I am indebted for the use of the finest specimen of it I have seen. I am also under obligations to C. B. Dyer, Esq., of Cincinnati, for the use of two smaller, and nearly as good specimens. Figures of the species, with a full description, will be given in the Ohio Geological Report.

Locality and position.—Mr. Case's specimen was found by him at Richmond, Indiana, in the upper part of the Cincinnati group, and those belonging to Mr. Dyer were found at about the same horizon between Cincinnati and Oxford, Ohio.

Lepocrinites Moorei *Meek.*

Body obovate. Base forming nearly one-fourth the height of the body, its four pieces being about as wide as long, of nearly equal size, and irregularly pentagonal in form, excepting one on the anal side, which is hexagonal. The five pieces of the second range of irregular form and size, two on the anal side being longer than wide, and extending up to form the lower margin of the principal opening, which is moderately large, and placed about one-third the length of the body below the top. Arrangement of the plates above not clearly made out. Pectinated rhombs four, comparatively large, one situated at the suture between one of the basal pieces and the contiguous piece of the next range above, on the anterior side of the body; another on the side to the left of the

* Now, since we know the nature of the vault of *Cyathocrinites* (see Proceed. Phila. Acad., N. Sci., Dec., 1868, p. 324 and 336), it seems to me that *Palæocrinus* cannot stand as a distinct genius from that group.

opening, and arranged with its longer axis directed transversely, on a line with the opening, while the other two are nearly on a line with the right side of the opening, on three plates that corner together, the arrangement being such that their longer axes diverge at right angles upward; in these, 15 to 20 of the little bars may be counted.

Recumbent arms short, or confined mainly to the upper side, one extending down nearly to the opening on the anal side, another to the two rhombs to the right of the opening, a third to that on the left, and the fourth to the anterior side, the direction of all being thus nearly or quite at right angles to each other. Column thick at the base of the body, but tapering rapidly below; as usual, composed of very thin pieces. Surface of body plates marked by distinct radiating lines.

Height of body, 0·46 inch; breadth, about 0·36 inch; thickness of column at its connection with the base, 0·14 inch.

This species seems to agree well with the genus *Lepocrinites* of Conrad, excepting in the very unusual character of having four rhombs, instead of only three. As one of these, however, seems to be merely rudimentary, or in other words, not perforated by little slits, I can scarcely think its presence a generic character.

I believe this is the first example of this group of *Cystidians* that has been found in the Lower Silurian, in this country. It occurs, however, in the upper part of the lower series, where some of the other fossils begin to resemble Upper Silurian types.

It has been proposed to correct the orthography of Mr. Conrad's genus to *Lepadocrinus*, and if this orthography should be adopted, the name of our species would be written *Lepadocrinus* or *Lepadocrinites Moorei*. The most usual custom, however, has been to retain the original orthography of generic names in such cases.

The specific name is given in honor of Prof. Joseph Moore, of ——— College, Richmond, Indiana, to whom I am indebted for the use of the only specimen I have seen.

Locality and position.—Upper part of the Cincinnati group, at Richmond, Indiana.

ANODONTOPSIS? MILLERI *Meek.*

Shell ovate, rather compressed or only moderately convex, the greatest convexity being a little above and slightly in advance of the middle, extremities more or less narrowly rounded, basal margin longitudinally semi-oval in outline, the most prominent part being near the middle; cardinal margin sloping from the beaks at an angle of 130° to 135° and rounding into the lateral margins; beaks only moderately prominent, somewhat obtuse, and not very convex, placed more than one-third the length of the valves from the anterior end. Surface smooth, or only with obscure lines of growth.

Length of a medium sized adult specimen, 0·83 inch; height, 0·59 inch; convexity, 0·30 to 33 inch.

It is not without considerable doubt that I refer this shell to McCoy's genus *Anodontopsis*, since it does not seem to correspond exactly in its hinge characters to his description of that genus, if I correctly understand him. As the hinge of *Anodontopsis*, however, has not yet been illustrated, and different authors do not always describe the same hinge exactly in the same way, I have concluded to refer our shell, for the present, provisionally to *Anodontopsis*. If a new genus, however, it may be called *Orthodontiscus*.

Prof. McCoy described the hinge of his genus as follows: "hinge line shorter than the shell, with a posterior long slender tooth or cartilage plate extending just below it (double in the right valve), and another similar but shorter one in front of the beaks," and then adds that there is "occasionally one small cardinal tooth beneath the beak."

In the shell here described, the hinge may be characterized as having one rather well defined, subtrigonal, or somewhat obliquely extended cardinal tooth under the beak of the right valve, and a corresponding pit under the beak of the left valve, with sometimes a slight prominence or rudimentary cardinal tooth just in advance of this pit; while of posterior lateral teeth there is in the right valve one long tooth ranging parallel to the cardinal margin, with a parallel furrow above and below it for the reception of two posterior laters in left valve, the lower one of which is more prominent, and the upper merely linear or rudimentary. The furrow between these two posterior lateral teeth of the left valve is well defined, and receives the tooth between the two furrows in the other valve. Below the lower of these furrows on the posterior side of the right valve, there is a very slight marginal ridge, that may sometimes assume the character of a second posterior lateral, but it is most prominent anteriorly, where it connects with the cardinal tooth, of which it seems rather to be an oblique posterior prolongation, than a distinct tooth. On the anterior side, there is one shorter anterior lateral tooth in the right valve, also ranging parallel to the hinge margin, and above and below this a little furrow for the reception of two small anterior laterals in the left valve, which receive between them that of the right valve.

The pallial line is certainly simple, and the muscular impressions well defined, the posterior one being larger than the other, and provided with a small accessory scar above just under the posterior ends of the posterior lateral teeth. The ligament or cartilage was probably small and internal, as there are no traces of an external ligament to be seen, the valves fitting close all along the hinge margin. No lunule or escutcheon is to be seen in any of the specimens.*

The specific name is given in honor of S. A. Miller, Esq., of Cincinnati, Ohio, who sent on to the Smithsonian Institute the first

* From all of the known characters of such extinct shells as this, I would be inclined to refer them to the family *Crassatellidæ*, instead of to the *Mytilidæ*, with which Prof. McCoy associates *Anodontopsis*.

specimens of this shell I have seen. I am also indebted to him for some broken valves showing the hinge. For the use of a good specimen showing the hinge of the left valve I am likewise under obligations to C. B. Dyer, Esq., of Cincinnati.

Locality and position.—Forty miles west of Cincinnati, Ohio, above the middle of the Cincinnati group, of the Lower Silurian.

ANODONTOPSIS? UNIONOIDES *Meek.*

This species has at least all the external characters of the genus, including the last, but nothing is known of the nature of its hinge. Specifically it differs from the species *Milleri*, not only in being very much larger than the adult size of that shell, but in having its anterior outline more regularly rounded, and its posterior obliquely subtruncated above, and with its most prominent part below the middle. Its ventral margin is also much straighter in outline; while its beaks are more depressed and placed decidedly nearer the anterior side, and its dorsal margin is not declining on the posterior side of the beaks as in the last. It likewise differs in having its posterior umbonal slopes more convex on a line from the beaks to the posterior basal margin.

Length, 1·73 inches; height, 1·11 inches; convexity, 0·63 inch.

Locality and position, same as last. The only specimen I have seen was kindly loaned to me for description and illustration, by Mr. S. A. Miller, of Cincinnati.

Remarks on the genus Lichenocrinus; by F. B. MEEK.

Perhaps of all the remarkable types of that protean order of animals known as the *Crinoidea*, there are few more curious and interesting forms (if really the *body* of a Crinoid) than that for which Prof. Hall proposed the name *Lichenocrinus*. Having recently had an opportunity to examine an extensive series of specimens belonging to both of the known species of this type, in the collections of Mr. C. B. Dyer and other gentlemen of Cincinnati, I propose to make a few remarks on the same, that may be of some interest to paleontologists, especially as this fossil is little known, and the specimens now obtained afford the means of giving a more extended description of its characters than that already published.

Prof. Hall's generic description of this crinoid reads as follows: "Bodies parasitic on shells and other foreign substances. Form discoid or depressed-convex, with a proboscidiform appendage rising from the centre. Disk composed of an indefinite number of polygonal plates, and apparently having no distinct mode of arrangement. Proboscis perforate, and in the known species, formed of five ranges of short plates alternating and interlocking at the margins."

From the specimens now known, the following more extended description of this fossil may be given:

Discoid or depressed-plano-convex bodies, growing firmly attached to shells, corals, trilobites and other marine objects, and

entirely destitute of free or recumbent arms or pinnulæ, ambulacral openings or pectinated rhombs. Free or convex side, concave in the central region, and composed of numerous small, non-imbricating polygonal plates, without any definite arrangement; mesial depression provided with a very long, slender, perforated, flexible column-like appendage, composed of five longitudinal series of short, alternately interlocking pieces. Attached side, when separated, presenting no sutures or openings, but in some conditions, showing numerous, distinct regularly arranged, radiating striæ, corresponding to radiating lamellæ that occupy the whole internal cavity from top to bottom.

Among the more remarkable features of this fossil, may be mentioned its very curious system of radiating lamellæ occupying the whole internal cavity, and giving it, when the plates of the upper side are removed so as to expose these lamellæ in place and attached to the adhering side, almost exactly the appearance of the little fungioid coral *Micrabacia*. The entire absence, so far as known, of free or recumbent arms or pinnulæ, as well as of the most minute ambulacral or other openings, save the minute perforation into the slender column-like appendage; and the attachment of this appendage to the free side of the firmly adhering disk, are also very anomalous features, if we view this disk as the body of a crinoid.

On examining one of these fossils, one of the first questions that suggests itself is, what can be the nature of this long slender appendage, not more than four to eight or ten hundredths of an inch in diameter, and several inches in length? Is it homologous with the so-called proboscis or ventral tube of other crinoids, or with the column of the same? Prof. Hall evidently entertained the former opinion at the time he wrote the diagnosis quoted above, though I was informed at Cincinnati, that after seeing other specimens than those from which his diagnosis was written, he inclined to the opinion that it is a column. That one or the other of these views is correct, would almost necessarily seem to be the case, and yet there would appear to be rather strong objections to both of these conclusions, if we view the disk as the body of a crinoid. In the first place, if a column, why should the body, instead of being, as usual, attached by it, be always (when not accidentally detached) found growing firmly by the whole opposite side to foreign bodies, and this long appendage in all cases be left dangling free, and if viewed as a column, apparently useless? Again, if a column, connected with the free side of the body of an attached crinoid, how are we to account for the fact that no traces of any other opening than that passing in through this appendage can be seen, even by a careful examination under a magnifier, in any part of the body? In addition to this, it does not connect with the disk by a series of basal pieces, as is usually the case with the connection of the column of a crinoid or cystoid to the body of the same, but on the contrary, the plates of the disk diminish in size inward, and pass by easy gradations into those forming the base of this long appendage.

On the other hand, if we proceed to view this appendage as a proboscis, or ventral tube, connecting with the ventral side of the body, we are met by the objection of its extreme proportional length, slenderness, flexibility, and the fact that it seems to taper off nearly to a point at its free end. In Mr. Dyer's collection there is a piece, apparently of the free end of this organ, about an inch in length, and agreeing exactly in size, form and structure, with that of *L. Dyeri*, that is broken at one end, and tapers to a slightly blunted point at the other end, which is composed of very minute pieces drawn together. In other examples, where three or four inches in length of this appendage can be seen attached to the disk at one end, it tapers off until it becomes exceedingly slender at the free broken end. This character of its termination, especially when viewed in connection with its length, slenderness and other characters, would seem to be a strong objection to the conclusion that it is a ventral tube or proboscis. Still there might have been a minute opening at the extremity, closed by diminutive pieces, as we often see is the case with the opening of much larger crinoids.

While examining the specimens of this type, several solutions of the mystery of its structure suggested themselves, the first one of which was, that possibly the disk, viewed as the body, might really be only a peculiarly constructed root, or base of attachment of a crinoid, the body of which grew at the free end of the long column-like appendage. This suggestion derives some support from the fact that the disk, although usually growing on the flat surfaces of shells, etc., is sometimes found growing upon the side of the columns of other larger crinoids, as well as on other uneven surfaces, and in such cases, it is bent around to conform to the curve of the surface of attachment, just as we see in crinoid roots similarly situated; while its whole interior is so filled with radiating lamellæ, as to leave extremely little, if any, space for the viscera of an animal, and is, as already stated, apparently hermetically sealed, excepting the minute canal leading up into the long appendage. It is true that the roots of crinoids are generally formed of thickened and anchylosed rings or segments of the column, but Mr. Billings has figured the root of one type (*Cleiocrinus grandis*), apparently composed of an accidentally folded expansion of minute polygonal plates; and it is worthy of note, that the column attached to this root is longitudinally divided by five sutures. It is also true, that there is no example, so far as known to the writer, of any such system of radiated lamellæ being connected with the root of a crinoid; but this objection would doubtless apply with even greater force against the conclusion that this disk is the body of one of these animals.

On the other hand, among the strong objections to the suggestion that these disks are roots, may be mentioned their very regular symmetrical form, and the fact that no indications of a body at the free end of the column-like appendage have yet been observed, nor of a detached body with adhering portions of a col-

umn agreeing with this; while no free criniod that might have been attached to this column in its early stages of growth, is known in these rocks. In addition to this, the tapering and pointed extremity of this appendage would seem to render it at least improbable that it had ever supported a body at that end.

Two other solutions of the difficulty suggest themselves, one of which is, that possibly the specimens, as we now see them, may not be the mature condition of the animal, but only one of the stages of development of some Crinoid, which, if known in its adult condition, is supposed to be an entirely distinct type. The other is that the disks, as we now see them growing fast to other bodies, may be the adult condition of a Crinoid that in its earlier stages of growth was supported on its little column, as in other types, being otherwise free, and that at a later period of its growth, the column became free at its lower end, and was, for a time, trailed about by the floating body, which finally inverted itself and grew fast to other objects by what was originally its vault. The fact, however, that these disks attain a diameter of at least half an inch, with the elongated appendage four inches or more in length, would, even, if known analogies supported such a view, seem to be a very strong objection to the conclusion that these are immature, or embryonic forms; while to say nothing of other strong objections that naturally present themselves against the last mentioned suggestion, the occurrence of these disks of all sizes, from the largest down to others less than a tenth of an inch in diameter, all alike growing fast to other bodies by the side opposite the column-like appendage, seems to demonstrate that this is their mode of growth from the first.*

In view of all that is now known of this curious fossil, it seems to me, without undertaking to express a positive opinion on the subject, that the weight of evidence (supposing that these disks are really the *body* of the crinoid) favors the conclusion that the long appendage is a ventral tube; but *if the appendage is a column*, then I should incline to the opinion that the disk is a peculiarly organized root, and that the body may be yet unknown, unless as an entirely distinct crinoid.

For the use of specimens of this fossil, I am under obligations to Mr. C. B. Dyer, Mr. U. P. James, Mr. D. H. Shaffer, Dr. H. H. Hill and Dr. R. M. Byrnes, of Cincinnati. Mr. Dyer's collection, however, contains much the most complete and instructive series. Full illustrations, showing all of its known characters, will be prepared for the reports of the Ohio Geological Survey. The two known species, *L. Dyeri* and *L. crateriformis*, occur in the Cincinnati group of the Lower Silurian, near Cincinnati, Ohio.

* In a few very rare cases, the disk has been found detached, and showing the flat side marked by very regular radiating striæ. It is almost certain, however, from the fact that hundreds of specimens have been found growing firmly to other bodies, that these few separated individuals had become detached by the disintegration of the object upon which they grew, and that the radiating striæ are only the edges of the lamellæ within, exposed by weathering, as we also sometimes see on the upper side of weathered specimens.

ART. XXXVIII.—*Discovery of a new Planet, and the Elements of the 114th Asteroid;* by Dr. C. H. F. PETERS. (From a letter to one of the editors, dated Litchfield Observatory of Hamilton College, Clinton, Oneida Co., N. Y., September 11, 1871).

ON the night of the 8th inst. a new planet was found, which probably will receive the number (116) of the asteroid group. The weather has favored me, and I have obtained the three following observations:—

	Ham. Coll. m. t.	A.R. (116).	Decl. (116).
1871, Sept. 8.	$15^h\ 33^m\ 34^s$	$0^h\ 14^m\ 6{\cdot}5^s$	$-3°\ 44'\ 38''$
9.	11 47 31	0 13 30·3	−3 48 48
10.	12 38 3	0 12 41·7	−3 54 12

These positions may be slightly modified by adopting more correct places of the stars of comparison. The planet is somewhat brighter than 11th magnitude.

Of the 114th asteroid (which has been named *Cassandra*), I have computed the following elements, from observations of July 28, Aug. 8 and 18:—

Epoch: 1871, Jan. 0. Berlin m. t.

$M_0 = 118°\ 5'\ 1''{\cdot}84$

$\pi = 148\ 29\ 23{\cdot}1$

$\Omega = 163\ 53\ 32{\cdot}3$ } Mean Equ. 1871, 0.

$i = 5\ 1\ 30{\cdot}05$

$\varphi = 8\ 51\ 32{\cdot}14$

$\mu = 817''{\cdot}54$

$\log a = 0{\cdot}4249978.$

These represent an observation of Sept. 9 to within a few seconds, and therefore must be very nearly accurate. From them we learn, further, that the planet is really not so very small, but only appeared so in its present opposition. For, we find that it is now in the remoter part of its orbit, near its aphelion.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS.

1. *Researches in Electricity:—Inaugural-Dissertation for the attainment of the Degree of Doctor of Philosophy at the Georg-August-University, Göttingen;* by THOS. R. BAKER, of Pennsylvania, U. S. A.—The prominent part of these researches was the comparison of the spark length of a given quantity of electricity with its density, and also the spark length with the quantity itself. The apparatus used was the Lane discharging jar, the sinus electrometer of R. Kohlrausch, and a large multiplicator

employed by Professors Weber and R. Kohlrausch in their determination of the magnetic and mechanical units of electricity. The sinus electrometer is an apparatus by which the density of electricity is determined by the efforts of a charged magnetic needle (a small magnet) against the horizontal intensity of the earth's magnetism. By means of the multiplicator, the quantity of electricity is estimated by measuring the influence of a current many times multiplied upon a small magnet within it.

The jar and electrometer combined were used for the first part of the investigation. The apparatus was charged, and then by means of a micrometer the negative was made to approach the positive ball of the jar until the spark appeared, and at this instant the position of the needle was noted.

The jar combined with the multiplicator served for the second part of the investigation. The balls of the jar were placed at fixed distances apart; and then the electrical machine, also connected with the apparatus, turned until the appearance of the spark.

The examination of the relation was made by seeking the straight line or curve whose equation the expressions for the elements of comparison in question regarded as coördinates most nearly satisfied.

From the results obtained it is concluded that the relations sought in both parts of the investigation most nearly approach that of the coördinates of the hyperbola, though that relation is not presented as *clearly the physical* law in the case.

The relation of the electrical condition of the atmosphere to its moisture, temperature and pressure, and the effect of motion upon the escape of electricity from the needle, were investigated at the same time. In the latter it was shown that the needle when in motion lost its electrical charge considerably sooner than when at rest.

Millersville, Pa., Aug. 14th 1871.

2. *Water unfrozen at a temperature of* $-18°$ *C.*—BOUSSINGAULT finds that by preventing the dilatation of water, it may be kept unfrozen down to $-18°$ C.. He experimented with a gun barrel of steel, into which a steel ball was dropped before filling it with water. During the cold days of December 26, 27 and 30, last, the temperature fell to $-12°$ and $-18°$, and yet on shaking the tube the ball was found to move freely, showing that the water was not frozen.—*L'Institut, July* 12.

II. GEOLOGY AND NATURAL HISTORY.

1. *Glaciers.*—Canon Moseley has a paper entitled, "On the Mechanical impossibility of the descent of glaciers by their weight alone," in the Philosophical Magazine for August.

2. *Time of the Glacial epoch.*—Lieut.-Col. Drayson in discussing before the Geological Society the "probable cause, date and duration of the Glacial epoch," starts from the fact that the pole of the ecliptic would be the center of polar motion as the pole

varied its distance from that center. He indicated the curve which the pole did trace, and this curve was such as to give for the date 13,000 B. C., a climate very cold in winter, and very hot in summer, for each hemisphere; the duration of the glacial epoch he fixed at about 16,000 years. He stated that the calculation resulting from this movement agreed accurately with observation.—*Phil. Mag. Aug.*, 1871.

3. *Das Elbthalgebirge in Sachsen*, von Dr. HANNS BRUNO GEINITZ. 1st Part. I. The Sea Sponges of the Lower Quader. 42 pp. 4to. with 10 plates. Cassel, 1871. (Theodor Fischer).—This valuable memoir by Dr. Geinitz notices or describes and admirably figures the following sponges: *Spongia Saxonica* Gein., *Cribrospongia subreticulata* Münster in litt., *Cr. isopleura* Reuss, *Cr. heteromorpha* Reuss, *Cr. bifrons* Reuss, *Plocoscyphia pertusa* Gein., *Amorphospongia vola* Michelin, *Sparsispongia varians* de From., *Tremospongia* (d'Orb.) *pulvinaria* Goldf. sp., *Tr. rugosa* Goldf. sp., *Tr. Klieni* Gein., *Cupulospongia* (d'Orb.) *infundibuliformis* Goldf. sp., *C. Roemeri* Gein., *Stellispongia Plauensis* Gein., *St. Reussi* Gein., *St. Goldfussiana* Gein.., *St. Michelini* Gein., *Epitheles* (de From.) *tetragona* Goldf. sp., *E. foraminosa* id., *E. robusta* Gein., *E. furcata* Goldf. sp., *Cherendopora undulata* Mich., *Ch. pateræformis* Mich., *Elamostoma* (de From.), *Normanianum* d'Orb. sp., *El. consobrinum* id. *Siphonia piriformis* Goldf., *S. annulata* Gein., *S. bovista* Gein. The memoir is a very important contribution to this department of paleontology.

4. *Sieboldtia Davidiana.*—M. Blanchard has described under this name a large Salamander from the north of China, which appears to be different from that of Japan.—*L'Institut, July* 12.

5. *Bivalve Crustaceans.*—A paper on recent Ostracoids, bivalve crustaceans, from the Gulf of St. Lawrence, by G. S. Brady, is contained in the Canadian Naturalist, No. 4 of Vol. V. It includes notices of 29 species, 5 of which are illustrated by 13 figures.

6. *On the early stages of Terebratulina septentrionalis*, by EDWARD S. MORSE, Ph.D. 12 pp. 4to, with two plates. A paper showing careful research, from the Memoirs of the Boston Society of Natural History, Vol. II, Part I, No. II.

7. *Glacier Scratches along valleys.* (Appendix to Art. XXXII.) —Prof. Emmons, in his N. Y. Geological Report (1842) at page 422, says that the direction of the glacier scratches in northeastern New York "conforms to that of the great valleys; in the Champlain valley it is north and south; in the St. Lawrence valley, southwest." The particular localities where his observations were made are not mentioned. He describes deep and broad channelings of the Trenton limestone west of Watertown, on Black River, which have a southwest course, and extending to the shore of Lake Ontario, a distance of ten miles; and states that the scorings of the rocks are parallel to the valleys, which here run southwest. Among western observations on this point not published in this Journal, are those of Prof. C. A. White in his Iowa Report, and those of Dr. Newberry.

8. *Anthers of Parnassia.*—In the Journal of the Linnean Society, vol. xi, Mr. A. W. Bennett published, two or three years ago, an interesting article upon *Parnassia*, its structure, affinities, and its mode of fertilization. I am now to remark only upon its anthers, which are generally described as extrorse. Mr. Bennett, observing that the present writer, in the Genera of North American Plants Illustrated, describes the anthers as introrse, and gives a drawing of *P. Caroliniana* as an illustration, proceeds to say: "I do not, however, find any other observer to agree with Professor Gray's observation in this respect, except two American botanists, Dr. Torrey and Mr. Chapman, who have probably borrowed their descriptions from him; nor do any specimens which I have been able to examine of this species confirm any departure in this respect from the ordinary type of the genus."

It is easy to show that Dr. Torrey's observation, at least, is independent and original. In his Flora of Northern and Middle States, published in 1824, p. 326, he described the anthers of *P. Caroliniana* as "incumbent;" in his New York State Flora, 1843, as "fixed by the base, introrse." The first volume of the Genera N. Amer. Illustrated appeared in 1848. This season I have, for the first time, had the good fortune to see both *P. palustris* and *P. Caroliniana* in flower, in the Botanic Garden of Harvard University, the former blossoming at the beginning, the latter at the close of August. The difference between the two species "in this respect" is obvious.

In *P. palustris*, the anthers are certainly extrorse as to insertion; but the line of dehiscence lateral, with introrse rather than extrorse tendency.

In *P. Caroliniana*, the anthers are quite as much introrse as extrorse as to insertion, and truly introrse for dehiscence. A transverse section removes all doubt, showing the connective or solid part to be posterior, and the anther to be as truly introrse as possible. A. G.

9. *Journal of the Linnean Society* (*Botany*), No. 65, commencing the 13th volume, contains several papers of some interest. Dr. Hance, of Canton, has discovered and describes the plant the root of which is the Lesser Galingal, an export from southern China. He makes it a new species of *Alpinia*, *A. officinarum* Hance, but very near Roxburgh's *A. calcarata*. It appears that the export of it is increasing: 112,000 pounds, valued at £478, left China in the year 1867; nearly 178,000 pounds in 1868; and 370,800 pounds, valued at £3047, in 1869. Dr. Hance also contributes an article on the Chinese Silk-worm Oaks: he concludes "that all circumstances would seem to conspire to render the culture of the oak silk-worm in Europe a sure matter of success, if properly set on foot and fostered." Mr. Hanbury contributes Historical Notes on the *Radix Galangæ* of Pharmacy; from which it appears that its introduction into Europe was due to the Arabs. Although the drug has lost the important place which it once held in medicine, being merely an aromatic stimulant, which might take the place of ginger, yet it is still largely consumed,

especially in Russia, where it is used for flavoring the *liqueur* called *nastvika*, as a cattle-medicine, and by the Tartars it is taken with tea.

Dr. Masters gives a Note on the Genus *Byrsanthus* Guill., and its floral conformation, suggesting an explanation of the singular arrangement of the glands and stamens, and indicating that two species have been confounded.

Rev. S. Matier discourses on Tamil Popular Names of Plants.

The brothers Tulasne contribute New Notes upon the *Tremellineous Fungi* and their Analogues, dating from the Western Coast of France in December last.

Mr. Neale contributes from S. Africa four papers, three of them upon certain Orchids and their Fertilization, and one upon the mode in which certain *Asclepiadeæ* are fertilized. The details are curious, but can hardly be condensed into an abstract.

Mr. Bentham, President of the Society, concludes the number with his paper on the styles of Australian *Proteaceæ;* illustrated by two plates. The flowers, seemingly arranged for self-fertilization, are really adapted for crossing, in some by a sort of dichogamy, the stigma being immature at the time when the pullen is deposited just around it; in others the stigma is curiously and variously protected or kept out of the way of the anthers (sometimes through the agency of a castrated stamen) until after the pallen of that flower is all shed. A. G.

III. Miscellaneous Scientific Intelligence.

1. *Twentieth Meeting of the American Association for the Advancement of Science, held at Indianapolis, Indiana, August* 16–21, 1871.—The satisfactory result of the Chicago meeting of the American Association in 1868, had, no doubt, its influence in deciding the choice of Indianapolis as the place for holding the recent session. The result seems on the whole to have justified the expectations of those who advocated the claims of the capital of the State of Indiana for this annual Congress of Science. The number in attendance is stated to have been about 200, or about the same as at Troy, Salem, and Chicago. The titles of papers entered are fewer, having been 78, against 143 at Troy, and 150 each at Salem and Chicago.

The Indianapolis meeting, under the presidency of Dr. Asa Gray of Cambridge, appears to have given much satisfaction to those present, among whom were many distinguished workers in science. His Excellency Governor Baker welcomed the Association to the hospitalities of Indianapolis and of the State of Indiana in a cordial opening speech, to which the retiring President, Dr. T. Sterry Hunt, replied in his usual appropriate and graceful manner.

The formal address of the retiring President was delivered on the evening of the first day, before a large audience. Its subject, "The Geognosy of the Appalachians and the origin of Crystalline Rocks," and its general scope, are stated on page 205.

An interesting feature of the Indianapolis meeting was an excursion on a grand scale to the coal fields of Indiana. The excursionists, numbering over four hundred, were carried in eight coaches. They visited at Knightsville the iron furnaces of the Watson Brothers, where they were sumptuously entertained at luncheon; also three other iron furnaces and several coal mines on the way to the town of Brazil, where they were welcomed by the citizens at Masonic Hall and dined with the usual complimentary addresses on both sides. The recent development of the coal known as block coal, due largely to the intelligent zeal of Prof. E. T. Cox, the State Geologist of Indiana, was the object of greatest scientific interest in the excursion. This coal is used in the raw state in the iron furnaces, and is said to be remarkably free from both sulphur and phosphorus. The excursion rested at Terre Haute, a town of 20,000 inhabitants, which seems to have exhausted every means to render the visit of the Association delightful and profitable. Here they passed the night, holding an evening general session at the Opera House. A popular lecture on the Winged Reptiles was delivered by Mr. A. Waterhouse Hawkins. On the following morning Dr. Gray delivered a lecture on the Fertilization of Flowers by Insects to a delighted audience; and after an early dinner, the party returned by special train to Indianapolis.

An excursion to Mammoth Cave in Kentucky was the closing act of the Association, after the adjournment of the Indianapolis meeting.

The Association, after enjoying the public and private hospitalities of Indianapolis and of the sections of the State included in the excursions, adjourned on the 21st, having voted conditionally to meet in San Francisco, California, in July, 1872, the final decision having been left with the Standing Committee.

The officers chosen for the next meeting are: President, Dr. J. Lawrence Smith, of Louisville, Ky.; Vice-President, Prof. Alex. Winchell, of Ann Arbor, Mich.; Permanent Sec'y, Prof. Joseph Lovering, of Cambridge, Mass.; General Sec'y, Prof. E. S. Morse, of Salem, Mass.; Treasurer, William S. Vaux, of Philadelphia.

The following are the subjects of the papers presented, and of the public lectures:

1. In General Session.

1. On Pterosauria; by B. Waterhouse Hawkins.
2. Fertilization of Flowers by Insect Agency; by Asa Gray.
3. On true Musical Intonation; by J. D. Tillman.
4. The Earthquake of October, 1870; by Charles Whittlesey.
5. On the Iron and Coal Interest of Indiana; by T. Sterry Hunt.
6. An Examination into the Laws of Development of Organic Types; by E. D. Cope.

2. In Section A.—*Mathematics, Physics, and Chemistry.*

1. The Daily Motion of a Brick Tower caused by Solar Heat; by C. G. Rockwood.
2. On the use of the Zenith Telescope for determination of Time; by J. E. Hilgard.

3. On the construction and verification of Metric Standards for the United States; by J. E. Hilgard.
4. Organic Identity of the Albumen and Endopleura of Seeds; by T. C. Hilgard
5. The Influence of the Moon on the Crust of the Earth; by Clinton Roosevelt.
6. On Chemical Equivalents; by S. D. Tillman.
7. On the Transmission of Heat; by S. D. Tillman.
8. The Relation between the Distances and proper Motions of the Stars; by T. H. Safford.
9. On the Earthquake of October, 1870; by Charles Whittlesey.
10. An inquiry concerning the Physical Relations between the Masses and Mean Distances of the Minor Planets; by Daniel Kirkwood.
11. On the Distribution of the Mean Distances of the Minor Planets; by Daniel Kirkwood.
12. Note on the Periodicity of the Solar Spots; by Daniel Kirkwood.
13. On the probable age of Halley's Comet; by Daniel Kirkwood.
14. Longitude Determination across the Continent; by George W. Dean.
15. On the Mutual Action of Electric Currents; by E. B. Elliott.
16. Radiation; by H. F. Walling.
17. The Chemical Equivalent of Æther; by H. F. Walling.
18. The co-relation of Electricity and Chemical Force; by H. F. Walling.
19. On a form of Boomerang in use among the Mogni Puebla Indians of North America; by C. C. Parry.
20. An improvement of Eggertz's Method of determining Carbon in Steel; by E. R. Taylor.
21. The Four Great Eras in Modern Astronomy; by Jacob Ennis.
22. Meteors; by Jacob Ennis.
23. The cause of Stellar Heat and Light; by Jacob Ennis.
24. The Character and Chemical Composition of the Meteorite that fell on May 21, near Searsmont, Maine; by J. Lawrence Smith.
25. A description of the exact locality of the immense masses of Meteoric Iron in Cohahuila, Mexico, with the analysis of one recently discovered; by J. Lawrence Smith.
26. A convenient and certain method of regulating a constant level of Water in the water baths of a Laboratory; by J. Lawrence Smith.
27. Remarks on the Cinnabar and other Minerals from California; by J. Lawrence Smith.
28. A new and ready method of making Platinum black; by J. Lawrence Smith.
29. A ready method of separating the Alkalies, on a large scale, from Lepidolite; by J. Lawrence Smith.
30. A new and convenient Specific Gravity Flask; by J. Lawrence Smith.
31. On the Enharmonic Scale of 31 tones in the octave, and a new practical keyboard corresponding with the accepted musical notation; by P. H. Van der Weyde.
32. On Oblique Microscopic Examination, and a new, simple apparatus for the same; by P. H. Van der Weyde.
33. On the use of the Balance for determining the changes in Atmospheric Pressure, and the co-efficient of Barometric Correctness; by P. H. Van der Weyde.
34. On the Invisible Caloric Extreme of the Solar Spectrum, and the non-caloric lines or bands in the same; by P. H. Van der Weyde.
35. On a new and more perfect fire test of illuminating Petroleum, without the use of fire; by P. H. Van der Weyde.
36. An application of an exponential function; by J. E. Hilgard.
37. To find a general formula for the length of "Curves of Pursuit;" by Joseph Ficklin.
38. Steam boiler Water and Incrustation; by Jos. G. Rogers.

3. In Section B.—*Geology and Natural History.*

1. The Monocotyledon the Universal Type of Seeds; by Thomas Meehan.
2. The Classification of Echinoderms from their Microscopic Structure; by Alexander Agassiz.
3. Mechanism of Flexion and Extension in Birds' Wings; by Elliot Coues.

4. On the Morphology of the Osseous System; by T. C. Hilgard.
5. On the Geological History of the Mexican Gulf; by E. W. Hilgard.
6. Observations on the Common Ground Worm; by James J. H. Gregory.
7. Observations on the Geology, Physical Features, and Retrocession of Niagara Falls; by George W. Holley.
8. Some Questions on Surface Geology; by Frank H. Bradley.
9. On the Entozoa peculiar to Swine; by William B. Fletcher.
10. On the Development of the Tarsal and Carpal Bones in Birds; by Edward S. Morse.
11. On the Characteristics of the Primary Groups of the Class of Mammals; by Theodore Gill.
12. On the Natural System of Fishes; by Edward D. Cope.
13. The Embryology of Chrysopa, and its bearings on the classification of the Neuroptera; by A. S. Packard, Jr.
14. On the Eozoon Canadense in the Crystalline Limestones of Massachusetts; by L. S. Burbank.
15. On the relation of Anomia; by Edward S. Morse.
16. Contributions to Physiographic and Dynamical Geology; by Richard Owen.
17. On the apparently one-ranked phyllotaxis of Baptisia perfoliata, and on the phyllotaxis of Cucurbitacea; by Henry W. Ravenel.
18. On the Geology of Northwestern Massachusetts; by Sanborn Tenney.
19. Western Coal Measures and Indiana Coals; by E. T. Cox.
20. Remarks on the Geology of the Mississippi Bottom; E. A. Smith.
21. Account of a Dust Storm which occurred in Clinton county, Indiana, Dec. 24, 1870; by Joseph Tingley.
22. Remarks upon the Cattskill Red Sandstone Group as it occurs upon the borders of New York and Pennsylvania; by James Hall.
23. Views of Nature: of the Organizing Principle, and of Life and Intellect; by E. C. Seaman.
24. Vitalism, Spiritualism, and Materialism; by E. C. Seaman.
25. The Eozoon Limestone of Eastern Massachusetts; by J. B. Perry.
26. Remarks on the Geological Map and Section of Missouri Rocks; by G. C. Swallow.
27. On the Extinct Tortoises of the New Jersey Cretaceons; by Edward D. Cope.
28. Remark on the Abies Douglassii, and a new species, or a peculiar variety of the Abies balsamifera, of the Rocky Mountains; by G. C. Swallow.
29. Remarks on the Snow Line in the Mountains of Montana; by G. C. Swallow.
30. On the Embryology of Amblystoma lurida of Sager; by P. R. Hoy.

In Subsection E.—*Archeology and Ethnology.*

1. A Theory on the Nature of the Difference in the Mental Capacity of High and Low Races of Men; by Renas Davis.
2. Note on the Distribution of Population in the United States; by J. E. Hilgard.
3. Rock Inscriptions in Ohio; by Charles Whittlesey.
4. An Ancient Mount on the Etowah River, Georgia; by Charles Whittlesey.

On the rates of interest realized to investors in the Securities of the United States; by E. B. Elliott.

Law:—What is it, and what are its Functions and Limits; by E. C. Seaman.

In Subsection C.—*Microscopy.*

1. Report on Photographing Histological Preparations by Sunlight; by J. J. Woodward.
2. Remarks on a new form of Achromatic Condenser, applicable to low and medium powers; by E. Bicknell.
3. On a new form of Micro-Telescope; by R. H. Ward.
4. Remarks on recent improvements in Achromatic Condensers; by R. H. Ward.
5. On the use of the Microscope in Chemical Analysis; by P. H. Van der Weyde.
6. On the observation of the Electric Induction Spark by the Micro-Spectroscope.
7. On Oblique Microscopic Illumination, and a new, simple Apparatus for the same; by P. H. Van der Weyde.

8. On some observed changes in Vorticella; by A. H. Tuttle.
9. Remarks on a Standard of Powers for Microscopical Objectives and Eye Pieces; by R. H. Ward.
10. On the Microscopic Structure of Eozoon Canadense; by E. Bicknell.

3. *On the relation of the Auroras to Gravitating Currents;* by PLINY EARLE CHASE, Professor of Physics in Haverford College. (Read before the American Philosophical Society, May 5th, 1871.) —Prof. Loomis's observations of the number of auroras in each month of 1869 and 1870 (Amer. Jour. of Science, III, S., i. 309), are specially noteworthy, both because of the careful accuracy of the observer, and because they are the first published observations which furnish satisfactory data for an approximate determination of the laws of auroral distribution.

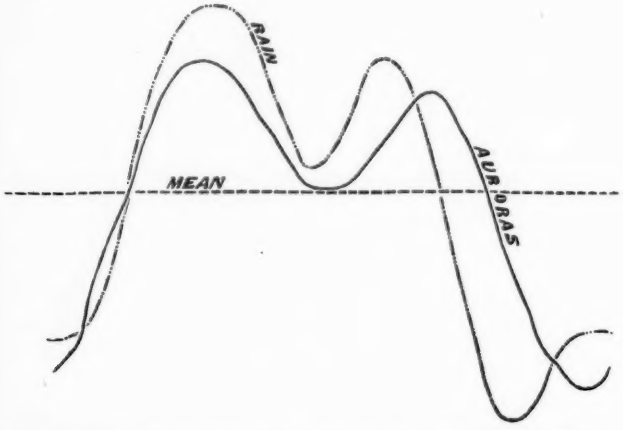
If the auroras are, as is now generally believed, luminous manifestations of terrestrial magnetism, it seems reasonable to look to them for some additional evidence upon the question of the relation between magnetic and gravitating currents. Messrs. Baxendell and Bloxam have already pointed out some resemblances between hyetal and magnetic curves (see Proc. A. P. S., x, 368), and if analogous resemblances can be traced between hyetal and auroral curves, they will be interesting and suggestive.

I have not found the similarity between the annual distribution of rain-falls and of auroras sufficiently striking to impress any one who has not made a special study of the causes of resemblance and difference. But, as I have repeatedly urged, currents are subject to an increased number of disguising disturbances, in proportion to the sluggishness of their motion, and the time which is consequently required for their formation or change. We may very reasonably look for analogies between the daily and the annual auroral or magnetic curves, of a character for which we could hope to find no parallel in wind, rain, or ocean-current curves.

If we desire, therefore, to find evidence of the joint influence of solar expansion and gravitating equilibrium, we should look where it is most likely to be found, and to the best of the observations which may be supposed to be fairly comparable. There are similar variations of solar attitude, and consequently increasing and diminishing solar force, in the day and in the year, but the effects of these variations upon the precipitation of vapor, are more likely to be shown in their greatest simplicity by the means of observations at different hours of the day than at different seasons of the year. I know of no published observations of this character at New Haven, but there are some extending over a long series of years at Philadelphia, and at Greenwich, the curves at each station indicating minima of rainfall at noon and midnight, and maxima in the morning and evening. The difference of longitude between Philadelphia and New Haven being less than $2\frac{1}{4}^{\circ}$, it is not likely that there is any material difference in the daily rain-curves at the two places.

In order to make the curves fairly comparable, both in regard to the times and the magnitudes of deviation, I treated the auroral

observations in the same manner as those of rainfall (Proc. A. P. S., x, 526). Both in the magnetic and in the hyetal phenomena, the greatest effects accompany the greatest atmospheric changes. But in the magnetic disturbances the principal maxima occur in the spring of the year and the morning of the day, while the general evaporation is increasing, whereas, in the daily rains at Philadelphia, the principal maximum occurs in the afternoon, when evaporation is diminishing. I have, therefore, compared the midwinter ordinate of the auroral with the noon ordinate of the rain curve, and the midsummer auroral with the midnight hyetal ordinate.



The auroral observations and the normal ordinates, of the accompanying curves, are given in the following table. I presume no one will doubt that the condensation of vapor, which is represented by the rain curve, is occasioned by the simple operation of gravitation in blending currents of different temperatures, and I see no reason for postulating any different law for the development of electricity and magnetism in the aurora.

Comparative Table of Auroras and Rainfalls.

Months.	No. of Auroras.	Normals.	Hours.	Normals of Rain.	Months.	No. of Auroras.	Normals.	Hours.	Normals of Rain.
		88	0	91			100	12	103
January	32	90	1	91	July	38	101	13	106
		94	2	93			103	14	109
February	31	98	3	98	August	34	105	15	108
		103	4	105			107	16	104
March	41	107	5	110	September	43	106	17	98
		109	6	113			103	18	92
April	44	109	7	113	October	38	100	19	87
		108	8	112			95	20	85
May	36	106	9	109	November	27	91	21	87
		103	10	105			89	22	90
June	31	101	11	102	December	30	87	23	91

IV. Miscellaneous Bibliography.

1. *War and the Weather, or the Artificial production of Rain;* by Edward Powers, C.E.—The object of this little volume is to show that rain can be produced by human agency, particularly by heavy discharges of artillery; and a large number of cases are cited in which great battles have been followed by speedy rain. Six cases of this kind are cited which occurred during our war with Mexico in 1846 and '47; nine cases of battles or skirmishes are cited which occurred in 1861 in the war of the rebellion, and which were followed by rain at no great interval; forty such cases are cited for 1862; thirty for 1863; twenty-eight for 1864; and six for 1865. Eighteen similar cases are also cited from among the great battles which have occurred in Europe during the past century, making a total of 137 cases. The author thinks that if these facts are insufficient to convince, it would be vain to expect to do so with a greater number of cases.

To this argument it may be replied that throughout the region from which his examples are mainly collected, rain falls upon an average once in three days, and probably a little more frequently; so that from the conclusion of one rain to the commencement of another, the interval is on an average but little over two days. Now battles are not usually commenced during a period of rain; generally not till some hours after the conclusion of a rain. Rain ought then generally to be expected within about one day after the conclusion of a battle. Now the argument of Mr. Powers is lame in this point. He takes no precise account of the length of the interval between the conclusion of a battle and the commencement of rain; nor does he show that this interval is less than it should be if the battle had no influence in the production of the rain; and in particular he takes no account of the cases unfavorable to his theory, in which rain follows a battle only after a very long interval. In order to make the argument complete, a much more careful analysis of the facts is required. It should be determined from a comparison of a large number of cases, including *all* the battles within a particular circuit, what is the average interval between a battle and the next succeeding rain, and it must then be shown that this interval is less than it would be if the battle had no influence in the production of rain.

The simplest mode of making the comparison may be the following: Determine for *all* the battles occurring within a particular circuit what is the average interval between the conclusion of the rain next preceding a battle and the commencement of the rain next following it; and then determine for the same region the average interval between two successive rains when no battle has occurred. If the former interval should be found sensibly less than the latter, it would be reasonable to conclude that the battle had exerted some influence in accelerating the fall of rain. The facts collected by Mr. Powers are not digested in any such manner; and although we are inclined to the opinion that great battles do exert some influence in the production of rain, we do not think

that Mr. Powers has established his proposition in a satisfactory manner.

With regard to the mode in which a heavy discharge of artillery might cause rain, we differ widely in opinion from the author of this book; but we are not fairly called upon to assign a reason *why* artillery firing causes rain until it has been shown that it *does*, at least sometimes, produce such an effect. We should be much pleased if Mr. Powers, or some other person, would resume the discussion of this subject in accordance with a truly scientific method. E. L.

2. *Introductory Text-Book of Meteorology*; by ALEXANDER BUCHAN, M.A., F.R.S.E., Secretary of the Scottish Meteorological Society. 218 pp. 12mo, with 6 plates. Edinburgh and London, 1870. (Wm. Blackwood & Sons).—Mr. Buchan takes the lead among the meteorologists and meteorological investigators of Scotland. This small and convenient text-book takes up in order the history and scope of Meteorology; Atmospheric pressure and its distribution over the globe—a subject which has been much elucidated through Mr. Buchan's labors; modes of observing and calculating temperature; solar and terrestrial radiation; distribution of terrestrial temperature and its relation to atmospheric pressure; moisture of the atmosphere; mists, fogs and clouds; rain, snow and hail; winds; storms; atmospheric electricity; whirlwinds, and waterspouts; aurora borealis and terrestrial magnetism; ozone; optical phenomena; meteors; weather and storm warnings. Six of the plates contain an exhibition for the globe of the isobarometric and isothermal lines; one is a synchronous weather-chart of Europe for 2d Nov., 1863, at 8 A. M., and one a similar chart of the West Indies for 1st Oct., 1866, at 8 P. M.

3. *Dominican Republic. Report of the Commission of Inquiry to Santo Domingo*, with the Introductory Message of the President, Special Reports made to the Commission, State Papers furnished by the Dominican Government, and the statements of over Seventy Witnesses: *Commissioners*, B. F. WADE President, A. D. WHITE, S. G. HOWE; Secretary, A. A. BURTON, Assistant Secretary, F. DOUGLASS. 298 pp. 8vo, with a map of the island. This volume contains reports from the Scientific view of the expedition, W. P. Blake, C. Wright and others. But the time at the island was too short for thorough exploration.

4. *Sun-Pictures of Rocky Mountain Scenery*, with a description of the Geographical and Geological features and some account of the Resources of the great West; containing thirty photographic views along the line of the Pacific Railroad, from Omaha to Sacramento. By F. V. HAYDEN, M.D., U. S. Geologist, Prof. Min. and Geol. in the University of Pennsylvania. 150 pp. 4to. 1870. New York: (Julius Bien). The character of this work has already been stated in this Journal, and citations have been made from its pages of important scientific observations. It is published in elegant style, the paper and printing being of the best kind; and the photographs excellent and highly interesting.